Price, One Dollar, Net.

THE

ABSOLUTE ATOMIC WEIGHTS

OF THE

CHEMICAL ELEMENTS

ESTABLISHED FROM THE ANALYSES
OF THE

CHEMISTS OF THE NINETEENTH CENTURY

AND DEMONSTRATING THE

UNITY OF MATTER,

PRESENTED IN SIMPLE LANGUAGE
TO THE

GENERAL SCIENTIFIC PUBLIC,

— BY —

GUSTAVUS DETLEF HINRICHS, M.D., LL.D.,

*Honorary and Corresponding Member of Scientific Societies in
Austria, England, France, Germany and the United States;
Professor of Chemistry in the St. Louis College of Pharmacy.*

WITH A PORTRAIT OF BERZELIUS AND THREE PLATES.

ST. LOUIS, MO., U. S.
CARL GUSTAV HINRICHS, PUBLISHER,
1901.

P. XV.

BERZELIUS.

PP. 84–5.

THE

ABSOLUTE ATOMIC WEIGHTS

OF THE

CHEMICAL ELEMENTS

ESTABLISHED UPON THE ANALYSES
. . . . OF THE . . . .

CHEMISTS OF THE NINETEENTH CENTURY

AND DEMONSTRATING THE

UNITY OF MATTER;

PRESENTED IN SIMPLE LANGUAGE
. . . . TO THE . . . .

GENERAL SCIENTIFIC PUBLIC,

—BY—

GUSTAVUS DETLEF HINRICHS, M. D., LL.D.,

Honorary and Corresponding Member of Scientific Societies in
Austria, England, France, Germany and the United States;
Professor of Chemistry in the St. Louis College of Pharmacy.

WITH A PORTRAIT OF BERZELIUS AND THREE PLATES.

ST. LOUIS, MO., U. S.
CARL GUSTAV HINRICHS, PUBLISHER.
1901.

Printed by R. P. Studley & Co., St. Louis, Mo.

I do not accept human or official authority, but exclusively depend on Nature. Page 229.

PROLOGUE.

The Atomic Weights of the Chemical Elements are the most important of all numerical values of Nature, both practically and theoretically.

Practically, these values are used daily in all the laboratories throughout the world for the reduction of analyses, and in the chemical manufactories for the estimation of the proportions of materials to be used and to check the product obtained.

If the atomic weights so used are in error, even the best made chemical analyses will necessarily be falsified by such error.

Theoretically, these atomic weights must be known with accuracy to permit the solution of the highest philosophical question of chemistry, namely, that of the Unity of Matter.

Let us briefly trace the work done in this field.

A century has passed since Dalton introduced the idea of atomic weights into the science of chemistry.

During the first half of that century, the great chemist of the North, Berzelius, made numerous excellent determinations of the atomic weights of all elements then known.

Berzelius himself, and his School which comprises many distinguished German Chemists, such as Rose, Mitscherlich and Wöhler, employed exclusively rigid methods, mainly dry way operations. No fancy work of any kind was tolerated. No pretenses to extreme accuracy were made, and the accuracy of the balance was never substituted for the skill of the chemists. No gnats were strained at while swallowing camels.

About 1860, Stas of Brussels, started in an opposite direction. The wet way silver chloride titration was made a fundamental operation. Nitrates and chlorides were interchanged. Large quantities of matter were operated upon. All weights were ostentatiously reduced to vacuum.

The use of large quantities and the extreme accuracy apparently attained had the effect intended. The leading official chemists, and through them, the academies and chemical societies, instantly accepted the published results and overwhelmed Stas with honors.

As a matter of course, the final conclusion of STAS was also accepted: the atomic weights of the chemical elements have no common divisor, and therefore, the chemical elements cannot have their origin in one primitive substance.

The expression of any doubt in either the accuracy of Stas' atomic weights or the truth of his philosophic conclusion, has for almost forty years been considered an evidence of lack of scientific intelligence. The Unity of Matter was pronounced a chimera.

The position of Stas and his school was very much strengthened by the elaborate, though very faulty, recalculations of his analyses by prominent chemists, such as Lothar Meyer and Ostwald in Germany, Julius Thomsen and Sebelien in Denmark, Van der Plaats in Holland, and Frank Wigglesworth Clarke in the United States.

The work of Clarke is properly considered representing even the Government of the United States. For Clarke is Chief Chemist of the Geological Survey, under the Secretary of the Interior; his recalculations have been formally endorsed by the Secretary of the Smithsonian Institution and published officially at the expense of the Smithson Fund; it has, finally, been sent out under the official frank as registered matter. The deficiency of the postal service—partly so resulting—is made up by Congressional appropriations.

The same author Clarke is also habitually sent by authority of the National Government and at public expense, as delegate to Congresses of Chemists, and put in charge of National Exhibits at home and abroad. This highest possible official consideration has enabled him to exercise a ruling influence in the American Chemical Society.

A critical examination of the work of Stas, and especially of the recalculations of Clarke, is therefore not only a difficult, but also an extremely hazardous undertaking; only the absolute conviction of duty to truth and science induced and sustained me in this work.

I found even enormous errors of reduction to vacuum committed by Stas, but overlooked by all his recalculators. I discovered systematic errors in his syntheses of silver and lead nitrate, which make it impossible to use these vaunted syntheses for any atomic weight determination whatever. The silver chloride process is unfit for such use because it takes a sort of body-guard of salt to keep the silver chloride down as a precipitate.

These results of mine were published in the Comptes Rendus of the Academy of Sciences of Paris, from 1892 to 1894, and more fully in my TRUE ATOMIC WEIGHTS of 1894.

Some of the most eminent Chemists and Physicists have admitted these critical results, which have also been taught in one of the most famous Universities of the world, and have been published with approval in chemical periodicals and special treatises.

In 1897, the Smithsonian Institution issued a second edition of Clarke's Recalculations, in which my work is grossly misrepresented. These false atomic weights of Clarke and the Smithsonian Institution have recently been forced upon the attention of the Committee of Revision of the United States Pharmacopœia for adoption.

I have, therefore, deemed it advisable to present my work in the simplest possible, most elementary form, so that every one interested in the great scientific and public questions involved may understand the issue.

Since the false science is backed by Government Authority and published by the means of the Smithsonian Institution, and sent free through the registered mail by official frank, I have endeavored to put this work at an almost nominal price within the reach of all.

This work is herewith most respectfully submitted to the General Scientific Public, which I believe is vitally interested in this scientific question itself, and in the questions of scientific morals and administration involved.

The question of science is one of the greatest ever raised, namely: Is the material universe composed of one single substance or of many chemical elements?

Our work shows that the unity of matter is established with greater certainty than any other principle of scientific philosophy.

By our true and absolute atomic weights, the almost universal practice of falsifying the results of chemical analyses by reducing them with false atomic weights, will be stopped.

All chemical analyses made for many years in our Government laboratories have been so falsified by the use of the false Smithsonian Atomic Weights of Clarke.

The question of scientific morals is this: Are official leaders in science, at home and abroad, presenting to and endorsing before the world, scientific data and doctrines, which by a little careful examination, they ought to recognize themselves as totally false and contradicted by experiment and observation, and which have been demonstrated false in my publications specified?

The question of scientific administration is naturally primarily addressed to the citizens of the United States, as follows:

"THE SMITHSONIAN INSTITUTION,
founded for the Increase and Diffusion of Knowledge among men per orbem," and
governed by a Board of Directors, appointed by the
CONGRESS OF THE UNITED STATES,
which has accepted the Trust Fund of the Englishman Smithson for such an Institution;
is it right and proper
for the Secretary of that Institution to issue and indorse a work which can only Increase and Diffuse ERROR and FALSE SCIENCE among men per orbem?

The proofs, that the work of Clarke specified is of such a kind are here most respectfully submitted in this book.

In these United States we never had a State Church, and we trust we shall never have that which has been one of the greatest curses to Europe; only by fearful sacrifices —of which the thirty years' war was merely an episode— Europe has partly relieved itself thereof.

But will a STATE SCIENCE be less harmful than a STATE CHURCH?

Have we not now in this country, quite a strong State Science supported by millions of dollars of public funds annually, and rapidly branching into all spheres of activity?

We universally blame the Inquisition to-day for having condemned Galileo in a *darker* age, and for having insisted upon the stability of the earth and the reality of the motion of the sun and stars around our earth. Yet this Inquisition acted in the performance of its duty, and shielded millions against a spiritual danger which they believed to be very real and very great.

Besides, the Inquisition had the testimony of the actual phenomena in their favor, for the visible motions of the heavens exactly conform to their verdict.

POOR ME + US

But the scientific Secretary of the Smithsonian Institution at Washington, in 1897, delares de facto, by indorsing and publishing the work of Clarke, that chemical action changes the weight of matter, which is not only contrary to all experimental evidence, but also contrary to a universally accepted axiom of philosophy and science.

The Secretary of the Smithsonian Institution declaring officially to be true that which was demonstrated false, and using the funds of that Institution and its franking privilege to distribute these gross errors and falsehoods among men per orbem—was not (as were the members of the Inquisition) acting in the performance of his duty, which demands that the Smithson Fund, in trust given our National Congress, shall be used for

> "the Increase and Diffusion of KNOWLEDGE among
> "men per orbem."

The absolute atomic weights presented in this our work, are true to nature, for they are based upon the analyses and determinations of the most reliable chemists and physicists of the Nineteenth Century, from Berzelius to Lord Rayleigh.

I most respectfully submit the evidence collected in this book to the General Scientific Public and to the Students of Chemistry throughout the World.

GUSTAVUS DETLEF HINRICHS,
4106 Shenandoah Avenue,
ST. LOUIS, MO., U. S. A.

September, 1901.

THE TWELVE MASTERS

WHOSE EXPERIMENTAL DETERMINATIONS CONSTITUTE THE FOUNDATION OF OUR

ABSOLUTE ATOMIC WEIGHTS

IN PARTS SECOND AND THIRD.

It is the experimental work of these Masters we make use of—their personal opinion (generally opposite of our own), or even their calculated final values of atomic weights (generally obtained by erroneous methods) are of no special interest in this study. The order is that of the principal publication.

BERZELIUS, JÖNS JACOB. Born August 27, 1779, at Westerlösa, Oestergotland; died August 7, 1848, at Stockholm, SWEDEN. "The Greatest Chemist of the World," see pp. 84-85. Portrait, see Frontispiece, representing him in early life.

1810-1830, LEAD, 74-91, 263. Also: Al, 226. As, 231. Ba, 235. Bo, 238. C, 101. Fe, 91-94. Fl, 242. Pd, 263. Pt, 116. Sn, 267. Te, 269-270. Rule, 3. Balance, 41-43. Methods, 49. Gnat and Camel, 83. Mention throughout this book.

TURNER, EDWARD. Born in Jamaica, 1798; died February 13, 1837, in London, ENGLAND. The first Professor of Chemistry, University College.

1833, CHLORINE, 97-99, 240. Also: Pb, 87, 89. Ba, 235-236.

DUMAS, JEAN-BAPTISTE. Born July 15, 1800, at Alais; died April 11, 1884, at Cannes, FRANCE. For years the leading Chemist of France, occupying the highest scientific and Government positions at Paris.

1840, CARBON-DIAMOND, 39, 101-105, 238, 262. Graphite, 48, 103. Also: Al, 226. Ba, 236-237. Ca, 106-107. Cd, 240. Fl, 243. Mo, 260. P, 263. Pb, 89. S, 264. Sb, 266. Si, 267. Sn, 267-268. Sr, 268. Wo, 274-275.

Balance, 40. Titrations, 55. Skill, 110-111. Mention, 138, 157, 211, etc.

ERDMANN, OTTO LINNÉ. Born April 11, 1804, at Dresden; died October 9, 1869, at Leipzig, GERMANY. Professor of Chemistry, University of Leipzig.

1844, MERCURY, 49, 61-63, 95-97, 257. Also: C, 102-104. Ca, 50, 106-107. Cu, 242. Fe, 93-94. Se, 267. Mention, 112, 138, 167, etc.

Marchand, Richard Felix. Born August 25, 1813, at Berlin; died August 2, 1849, at Halle, Germany. Professor of Chemistry, University of Halle.

1844, Sulphur, 96-97. Also: Mg, 108. Wo, 273. With Erdmann: C, Ca, Cu, Se. Mention, 167, 211, etc.

Svanberg, Lars Frederik. Born May 5, 1805; died July 16, 1878, at Upsala, Sweden. Professor of Chemistry, University of Upsala.

1844, Iron, 91-95, 242. Also: Hg, 98. Mention, 112, 138, 211, etc.

Scheerer, Karl Johann August Theodor. Born August 28, 1813; died July 19, 1875, at Dresden, Germany.

1850, Magnesium, 50, 108-109, 114, 260. Mention, 138, 211.

Crookes, Sir William. Born June 17, 1832, at London, England. Founder of the Chemical News. Researches on Radiation in High Vacua. Discovered Thallium, 1861.

1873, Thallium, 120-138, 271, 281-4. Also: 89, 139-140, 169, 181, 209, 214, 218, 281, 294.

Seubert, Karl Friedrich Otto. Born April 6, 1851, at Karlsruhe, Baden. Professor of Chemistry at Tübingen, now at Hanover, Germany.

1881, Platinum, 115-119, 264. Also: Ir, 258. Mo, 260. Os, 262. Rh, 264. Mention, 50, 75, 138, 211.

Ramsay, William. Born October 2, 1852, at Glasgow, Scotland. Professor of Chemistry, University of London, and University College, London, England. Nullovalent Gases in the Atmosphere.

1893, Boron, 141-151, 238. Also: 33, 42, 43, 53, 63, 123, 211, 217.

Aston, Miss Emily, B. Sc. (Univ. of London). Associated with Professor Ramsay in his great work on Boron, 1893. England.

1893, Sodium, 147-148, 261.

Rayleigh, Lord. Born November 12, 1842, at Langford Grove, Essex, England. Successor to Maxwell at Cambridge, 1879. Professor of Natural Philosophy at the Royal Institution of Great Britain, an Institution without a peer in the world (maintained by a Society, and entirely independent of the Government). Discoverer of Argon.

1895, Nitrogen, 159-165, 201, 260. Hydrogen, 244. Also: Ur, 272-273.

CONTENTS.

PART FIRST.

THE ERRORS OF PRECISION IN ATOMIC WEIGHT DETERMINATIONS.

PART SECOND.

THE ABSOLUTE ATOMIC WEIGHT OF TEN LEADING ELEMENTS.

PART THIRD.

THE ABSOLUTE ATOMIC WEIGHTS OF BORON AND NITROGEN.

A. Experimental Determinations.

II. THE ATOMIC WEIGHT OF SODIUM, ASTON.

The Work Done, 147. Confirmation of $Cl = 35.5$, 148. The Silver Chloride Process Tested, 149.

Determinations by Henry Gautier, 152. My Carbide Process, 153. La Pleiade de Chimistes d'Alsace, 154. Monsieur Henri Moissan, 155. Friedel and Schützenberger, 156.

On the True Atomic Weight of Boron, Hinrichs, 158.

III. THE ATOMIC WEIGHT OF NITROGEN, LORD RAYLEIGH:

Statement, 159. Density Determinations and Atomic Weights, 160. Atomic Weight of Nitrogen, 1882, 160. Lord Rayleigh's Discovery, 161. Density Recognized for Atomic Weight Determination, 163. Why did not Clarke Drop Stas at the End? 164. Density Determinations by Leduc, 166. Leduc's Atomic Weights, 167.

B. The Folly and Fraud of Stas and his School.

Determinations by Chemical Means, 169.

I. THE CHALLENGE OF STAS:

The Challenge, 170. Syntheses of Silver Nitrate by Stas, 174. Table of Results, 177. Postscriptum, 181.

II. THE ATOMIC WEIGHT OF NITROGEN BY CHEMICAL MEANS:

Taken according to Stas and Clarke, 182. a. Synthesis of Silver Nitrate, 186. b. The Silver Nitrate and Potassium Chloride Ratio, 191. c. The Meanest "Mean" and its Impossible Probable Error, 195.

III. CHEMICAL ACTION CHANGES WEIGHT OF MATTER:

The Old Axiom, 198. Clarke's Grand Discovery, 199. The Author's Confession, 201. Die Erhaltung des Stoffes, Ostwald, 203. The Two Penitent Brothers, Ostwald and Hinrichs, 204. Ragnarok, 204.

IV. HERESY IN THE CHURCH OF STAS:

Clarke ignores Landolt, 205. How does Ostwald get Manuscripts, 206. The great Result of Landolt, 207. Annihilated by Clarke, 207. Si Quæris, Circumspice, 207. The Conflict Landolt-Clarke, both Stasians, 208. Reductio ad Absurdum, 209.

C. Conclusion.

PART FOURTH.

TABULAR VIEW OF THE ATOMIC WEIGHT ANALYSES OF THE NINETEENTH CENTURY.

ADDENDA TO PART SECOND.

CONCLUDING REMARKS.

SUPPLEMENTS.

ILLUSTRATIONS.

ERRATA.

No serious errata, requiring special mention, are believed to be present.

PART FIRST.

The Errors of Precision in Atomic Weight Determinations.

I. GENERAL INTRODUCTION.

By the new term of *Absolute* Atomic Weight we designate the *exact number* expressing the atomic weight of any element, that of *carbon-diamond* being taken at *twelve exactly.*

We are well aware that it is commonly considered impossible to determine this exact atomic weight. But we believe this opinion of chemists of the present due to their habit of considering the determinations of atomic weights a purely laboratory operation only.

The determination of the atomic weight of any element involves, however, two entirely distinct problems, the one chemical, the other mathematical.

The Chemical Problem.

The chemical problem consists in the production of the material, its chemical change into some other definite form, and the accurate determination of the ratio of the weight of these two materials.

Such a process we have in the reduction of pure lead oxide to metallic lead when moderately heated in a current of dry hydrogen.

This is one of the fundamental processes devised by Berzelius.

In the chemical symbols, also devised by him, the above oxide is represented by PbO.

The weighings give the ratio $PbO : Pb = a$, from which, for $O = 16$ assumed, the value for the atomic weight is easily calculated.

The material used should be absolutely pure, the change absolutely complete and without loss, and all weighings absolutely exact.

Strictly speaking, it is impossible to produce any material in an absolutely pure condition. It is equally impossible to completely convert any such substance into some other form of combination, without loss of any particle of matter, or without the accession of any particle of some other composition or nature.

Hence the chemical work required in atomic weight determinations can only be done approximately, not with absolute accuracy.

In other words, the chemical or laboratory work can only give us the atomic weight affected with a certain *minute error*, which this chemical work in any given single case is unable to determine.

By varying the chemical process involved in the determination, we therefore find *different values* for the atomic weight of the same element, because the minute chemical errors involved are necessarily different in the different processes.

Every page of this book, giving the record of all the chemical determinations actually made during a century, will show these differences, which often are quite considerable in amount. We shall as an example for this introductory exposition, give all the results obtained by Mallet for the atomic weight of gold.

For these reasons, and also simply as a matter of fact and record, the mere laboratory work of the chemist cannot give us the true or absolute value of the atomic weight of any element. The empirical values so obtained we shall subject to a thorough but a very simple mathematical examination to deduce from these conflicting empirical values the true atomic weight, freed from the unavoidable errors of the laboratory work.

The Mathematical Problem.

The theoretical or mathematical problem to be solved in order to obtain the true or absolute atomic weight from the varied values furnished by the chemical laboratory work, has hitherto received only a formal treatment in the use of the *mean* and its *probable error*. We must briefly consider this formal study before we can give the outline of our own general solution of the problem.

The Mean.

Chemists have, thus far, supposed that the *mean value* of the individual chemical determinations is nearer the true value sought for than any of the actual determinations made.

This *mean value* is calculated by summing up all individual values found and dividing this *sum* by the *number* of determinations made.

But what evidence do we possess proving that the average of the individual measures is the true measure? Absolutely none at all.

On the contrary, every chemist will readily admit that some chemical operations necessarily give values always too high, while others give them always too low. The great master Berzelius has already recognized this fact, and made it the basis of his most admirable rule of procedure. True At. Weights, p. 16.

"Try to find that method of analysis, in which the "accuracy of the result will depend to the least extent on "the skill of the operating chemist; and when this method "has been selected, then consider what unavoidable con-"ditions are present which may cause errors in the result, "and ascertain whether they will increase or diminish the "same. Thereafter make another determination, in which "exactly the opposite effects only can be produced. If the "result remains the same, the determination was correct." Sebelien, p. 13, quoted from Gilbert's Annalen, vol. 18, p. 537; 1814.

This rule of Berzelius points to our *higher and lower limits*, between which the true atomic weight must fall.

We must most positively protest against the universal assumption that the mere arithmetical *mean* of the experimental chemical determination is the true value of the atomic weight of any element.

But since this declaration is in conflict with universal practice, it may be advisable to first present a simple case from common experience in which the fallacy of the mean is palpable, and about which there cannot be any doubt.

As such we shall give the case of the determination of the weight of a coin of given value—a common silver dollar—by actually weighing a number of such coins in circulation.

Having done this work, we shall be able to understand why the *mean* cannot be taken as the true value in most cases.

We shall thereby be relieved from one of the most common errors of modern scientific practice.

It is impossible to overlook the glaring error of the mean, since according to the object or cases operated upon, the resulting mean values do differ from one another, as a matter of fact. See the atomic weights of gold determined by Mallet, pp. 24–28.

The Probable Error.

In order to obtain a sort of indication of the reliability of such a mean, chemists have tried to determine the "*probable error*" of such means by an application of general principles of probability put into practical form by mathematicians.

We shall therefore, be compelled to show how this simple though somewhat tedious calculation is effected, and above all, call attention to the *necessary limitations* imposed by mathematical science, under which alone this method can possibly be used.

We shall see that these necessary limitations have been overlooked practically in all the applications of the calculation of the probable error made by chemists, both in America and in Europe.

To what an extent this reckless use of a method under conditions where it is absolutely inapplicable has been carried, will be shown when we come to examine the *Constants of Nature* published by the Smithsonian Institution.

We shall incidentally also be compelled to show, that under Moissan this method has finally found its way into the most renowned Chemical Laboratory of Paris—and this fact is specially pointed out in a formal report of the entire Chemical Section of the Academy of Sciences of Paris, which granted a prize in accordance with this report, in December, 1900—when as a matter of fact even the simple formula for this calculation is not known in that Laboratory, so that all values calculated are fifty per cent too high!

We shall therefore have to show this calculation in detail, by applying it to our weighings of the silver dollars.

The Constant Errors.

We shall then also see clearly that this method does not furnish any clew to the limit of the constant errors of the chemical work, or any indication of the real error committed—in which opinion chemists have used it—but simply gives mathematical expression of the degree of concordance of the experimental determinations made.

It would indeed be delightful to possess some mathematical process by which erroneous chemical work could be corrected.

Chemists evidently thought that the calculation of the probable error shows them at least how near they came to the truth.

That this absurd fallacy could become accepted so widely in the chemical world is very deplorable; but that chemists, like Moissan, do not even take the trouble to use the true formula, is almost incredible; unhappily it is true

Two Common Errors.

We shall next find, that two additional and very grave errors are commonly committed in the so-called reductions of experiments or observations by chemists and other scientists, and which errors must be avoided to obtain true results of atomic weights.

We refer to the habit of calculating decimals beyond the limit of actual precision, and to the use of auxiliary data which are not true.

Suppose I can distinguish only the fiftieth of an inch, by sight, can I then pretend to accurately measure to the thousandths? And again, if I weigh to the centigramme because my balance requires at least half a centigramme of overweight to turn distinctly, how can I pretend to know the tenth of a milligramme by weighing with that balance?

And if, for calculating the cost of a given number of things, I use a false value per unit, will not my calculated price be false also, and therefore worthless?

We shall see, that also this kind of error is constantly committed by chemists, in their "adopting" as true some determination made by men in authority, without exacting proper proof that the authority did commit no error in his work.

Our Course of Training.

When we shall have become familiar with the reality of these errors, and shall have learnt how universally these very errors have been and are being committed by chemists in atomic weight determinations, we will have completed that introductory course of training necessary to the beginning of the study of the chemical and simple mathematical methods that must be used to obtain the true and absolute atomic weights of the chemical elements.

We shall now present each of these common methods and their common errors in the simplest way possible, by actual examples, and with the necessary details.

In order that this preliminary work may not become too tiresome, we shall freely call spade a spade, independent of the hand that uses it.

Our object being to establish truth, we shall not compromise with error, even if that error be practiced by men in the highest stations.

II. THE MEAN WEIGHT OF A SILVER DOLLAR.

In order to test the value of the mean, I have, on ten different days, taken a roll of twenty silver dollars at the bank—thus obtaining two hundred single silver dollars as they were in circulation during the first four months of 1901.

Each coin was separately weighed exactly to the centigramme and the mean or average weight was calculated, as well as the so-called probable error of this mean. This probable error we shall explain later on when we shall begin the study thereof; pp. 11–20.

It will not be necessary here to give the individual weighings except for the heaviest and lightest coin of each series of twenty, that is, the *extremes*. It is also important to notice the *range* or difference between these extremes.

We may here add, that the extremes and range furnish a true indication of the practical concordance of any series of determinations of any single value. We shall see further on, that this is all the probable error can do.

Determination of the Weight of a Silver Dollar.

1901.	No.	Highest.	Lowest.	Range.	Mean.	Prob. Error.
Jan'y 15,	20	26.69	25.98	0.71	26.40	3.2 cgr.
" 25,	20	6.71	6.10	.61	.48	2.6
Feb'y 8,	20	6.77	5.92	.85	.43	4.5
" 27,	20	6.76	5.84	.92	.36	4.2
March 11,	20	6.72	5.87	.85	.40	3.6
" 25,	20	6.71	5.86	.85	.31	4.1
April 4,	20	6.73	5.95	.78	.51	3.0
" 15,	20	6.71	6.02	.69	.37	3.7
" 25,	20	6.74	5.73	1.01	.28	4.8
May 3,	20	6.70	5.65	1.05	.33	4.2
Means,	20	26.72	25.89	0.83	26.39	3.7
Absol. extremes,		26.77	25.65	1.12		

It will be noticed that the mean weight of the silver dollar in any one roll of twenty coins varies from 26.28 to 26.51 grammes or 23 centigrammes; that is nearly a quarter of a gramme, or almost one per cent of the mean weight of the two hundred single silver dollars weighed.

The range in any one series of twenty silver dollars varies from 61 to 105 centigrammes, averaging 83 centigrammes for each series of twenty coins.

Mean Weight not True Weight.

But can we for a moment accept the mean weight of the two hundred silver dollars actually weighed as the true weight of a United States Silver Dollar? Is the mean 26.39 grammes actually determined from two hundred weighings, the true weight of our silver dollar? *Is the mean value of the means of ten series of twenty determinations each the* true weight?

Most assuredly not; nor would we obtain that true weight by indefinitely continuing this work of actual weighing the coins in circulation.

We have here a plain case showing the fallacy of accepting the mean value as the true value, even if determined by ten series of twenty experimental determinations each.

Effect of Wear. Abrasion.

In this case the cause of the error of the mean is well understood: it is due to the wear or *abrasion* produced by circulation, and this is not equal for the different coins but varies according to the actual handling each coin has undergone since leaving the mint.

And the amount of this abrasion will roughly depend on the length of time the coin has been in circulation, which time is determined by the year of coinage stamped on each coin.

It would be exceedingly interesting to give our full data of observation on this subject, an account of its importance on the common scientific practice of taking the mean value as the true value; but our space will allow only the following general points to be stated:

Frequency in Circulation.

The frequency of coin of any given year is most remarkably different, and not at all equal, as might have been supposed.

The 200 silver dollars weighed bear as years of coinage numbers from 1878 to 1899, extending over 22 years. Sixty one of these coins bore one of the three years 1889, 1890 and 1891; their mean year of coinage therefore is 1890. Sixty eight of the 200 silver dollars weighed were coined in the six years from 1879 to 1884 inclusive.

In the nine years here specified, 129 of the 200 silver dollars were coined, leaving only 71 coins to the 13 years not specified.

Roughly speaking we may say that in the number of coins in circulation the *three* years 1889-1891 and the six years 1879 to 1884 and all the other *thirteen* years not herein included have furnished a nearly equal number of silver dollars. namely respectively 61, 68 and 71 for each of these groups, This gives about 20, 10 and 5 for each single year of the groups of years specified.

This is a much greater variation in frequency than could have been anticipated.

The Mean a Lower Limit.

Since evidently abrasion lowers the weight of a coin in circulation, every weight of a coin is below its true legal weight and every mean will therefore also necessarily be below the weight fixed by law for the silver dollar coin (within the tolerance).

In this case, the mean weight of the actual coins in circulation can never furnish the true weight of the silver dollar.

The true weight of the silver dollar at the time of coinage is evidently the mean weight in actual circulation increased by the loss due to abrasion in circulation.

In other words, the actual weight determined by weighing the coins in circulation, and any means of such weighings, give only a *lower limit** of the true weight of the coin.

Amount of Abrasion.

Now, the 68 silver dollars coined between 1879 and 1884 gave the mean weight 26.288; their mean year of coinage is 1881½.

---

* On that fact rests our method used in the True Atomic Weights, 1894.

The 61 silver dollars coined between 1889 and 1891 gave a mean weight of 26.405 grammes; 1890 is their mean year of coinage.

The mean weight of these 61 dollars exceeds the mean weight of the 68 which are 8½ years older, by 0.117 grammes; hence the abrasion was at the rate of 0.137 in the ten years, between 1880 and 1890.

Calculated Weight of New Coin.

If we were permitted to assume the same amount of abrasion during the ten years from 1890 to 1900, we would fix the *mean weight* of a silver dollar at the mint in 1900, before entering into circulation, at 26.54 grammes, namely, to the mean weight, 26.405 for 1890 we would add the abrasion 0.137 found for ten years in the eighties.

Since now the abrasion of new coins is not necessarily the same per year as the abrasion for older coins that have already lost the most prominent points by abrasion, this calculated weight of 26.54 as the mean weight of a new silver dollar coined at our mints in 1900 is only a lower limit itself.

In that mean weight the tolerance will figure as an equally possible variation above and below the mean

Testing the Result.

I was unable to secure at banks and even at the U. S. Subtreasury in the City of St. Louis any silver dollars that had not yet been in public circulation.

But the legal weight was stated to be 412.50 grains, which is equivalent to 26.730 grammes. Hence we see that an estimate from the mean weights is still too low by 19 centigrammes.

Even the *means* of the highest observed is still one centigramme below the legal standard.

But in four of the ten lots of twenty silver dollars, the heaviest exceeded by a few centigrammes the legal standard.

It is well understood that it is impossible to produce coins of the exact weight fixed by law; a practical limit is assigned, called the "*tolerance.*"

Not having been able to obtain new silver dollars, even at the United States Subtreasury in St. Louis, I cannot determine the value of the actual tolerance practiced in coining our silver dollars.

A single stray new silver dollar of 1901, a *rara avis* in the West, was just trapped and found to weigh 26.77 grammes, which is 4 centigrammes above legal weight.

Estimate from Quarters.

But I succeeded in getting absolutely new quarters, at the Boatmen's Bank. The mean weight was 6.250 grammes; the heaviest 6.30, the lightest 6.20, showing a tolerance either way of 5 centigrammes.

If we could be permitted to take the new quarters as *one fourth* of a new silver dollar, the latter would weigh 25.000 grammes and show a greatest tolerance of 20 centigrammes either way.

But this mean is 1.73 grammes below the legal standard; accordingly we must suppose that the legal weight of a quarter is considerably less than the fourth of the legal weight of the silver dollar.

This shows, how complex even so simple a case as the experimental determination of a common silver dollar coin becomes when tried, without reference to the law governing the coinage.

Now, chemists have tried to experimentally determine the weight of the atoms—without reference to the general Laws of Nature. No wonder they made a mess of it, and now want to settle it by vote.

III. THE PROBABLE ERROR OF THE MEAN.

Scientists hold, that the *probable error* of a *single observation* is at such a distance from the mean that it is an even wager or an even chance for a single actual observation to fall within this distance or beyond it.

In other words, if we arrange all observed values in the order of their magnitude, *one half of all* should fall nearest the mean and be not more distant therefrom than is measured by the probable error of a single observation.

The *probable error* of the *mean* of a number of observations is obtained by dividing the probable error of a *single observation* by the square root of the total number of observations.

Thus, if 4 observations have been made, all with equal care, the probable error of the mean will be only *one half* of the probable error of a single observation; for 16 equally careful observations, the probable error of the mean will be only *one fourth* of the probable error of a single observation or determination.

In other words, mathematicians have demonstrated, that the *probable error of the mean* diminishes as the square root of the number of determinations *increases*.

In this circumstance lies the temptation to the belief that we need only increase the number of determinations to get nearer the truth.

That is, if this mean really were the true value. But we have seen the mean is not necessarily the true value.

Systematic and Constant Errors.

We cannot here enter upon this rather difficult discussion; we need only say, that all this very nice theory is rudely destroyed by the actual existence of systematic and constant errors, which in the above mathematical theory are *supposed* to be absent or to have been determined.

This is exactly as in the laws of the pulley in physics; very simple, easily understood, if friction and the stiffness of cordage are supposed not to exist; but we know, that these great influences can not be overlooked by us, because they constitute great facts in nature.

Calculation of Probable Error.

But since this method is in actual use, we shall have to give the method of calculation of the *probable error* of the *mean* of any number of determinations.

If, at any time, the *probable error of a single determination* be wanted, we can obtain it by multiplying the probable error of the mean by the square root of the total number of determinations, as practically stated above.

The manner of calculating the *probable error of the mean* is quite simple. Having calculated the *mean* of all determinations, we find the differences between this mean and every single determination. We square each one of these differences, and take the *sum* of these squares.

Next we multiply the *number* of determinations by the next lower number. The sum of the squares is divided by this product.

Two-thirds of the square root of the quotient thus obtained is the probable error of the mean.

It may be necessary to give an example in full detail of this calculation of the probable error of a mean. Let us take the 20 silver dollars weighed on April 4, 1901.

The year of coinage of each silver dollar and its weight (in grammes and centigrammes) is given in the first two columns.

The mean 26.51 gives the difference expressed in centigrammes in the next column. The fourth column gives the square of each of these differences.

Year.	Weight.	Difference.	Square.
1880	25.95	56	3136
82	26.24	27	729
90	.24	27	729
89	.37	14	196
78	.39	12	144
87	.40	11	121
89	.45	6	36
89	.45	6	36
84	.48	3	9
80	.51	0	0
97	.59	8	64
99	.59	8	64
96	.65	14	196
99	.65	14	196
97	.66	15	225
83	.68	17	289
90	.69	18	324
99	.71	20	400
91	.72	21	441
91	.73	22	484
	Mean 26.51		Sum 7821

The *sum* of these twenty squares foots up to 7821. The 20 observations multiplied by the next lower number (here 19) gives the *product* 380.

Dividing the sum 7821, by this product, 380, we obtain the *quotient* 20.58.

Extracting the *square root* of this quotient we obtain 4.54.

Subtracting one third herefrom, there remains 3.03 as its *two-thirds*, which therefore is the *probable error* of the mean of the twenty silver dollars, in centigrammes. We drop the second decimal as unreliable.

The operations involved in this calculation of the probable error of the mean are all simple enough—although quite tedious if a large number of such calculations has to be effected.

Shall we use this Error?

If the so-called probable error possesses any scientific value, it will then be proper to calculate the same. But if the value so obtained is practically worthless it would be worse than pedantry to carry out these calculations.

If the so-called probable error of the mean should convey a false idea, or have been obtained in any case under conditions which prohibit this mode of calculation, then *false data of fact* would be foisted upon science, and a *fraud* would be committed.

So far as science is concerned, the fraud would exist, even though the person guilty be not aware thereof—on account of lack of understanding.

In science, there can be no excuse given for stating a false fact or a false result obtained by using a false method or process, whether of practical laboratory work or of calculation.

It is the duty of the scientist to test the methods of practice and of calculation which he employs. If he continues to employ them after they have been shown to be erroneous, he is surely guilty of committing a scientific fraud in using them.

Condition by Number.

Now, first of all, this method of calculation *presupposes* that the number of observations or determinations is large. In our case it is 20; that is about as low as may be permitted.

But in the applications of this method made for the calculation of the atomic weights, generally but few data or determinations are at hand. In our record following this number will be stated in every case. It is generally under ten, mostly under five.

In one of the most favorable cases, that of lead, we find the number of determinations to be 9–4–3–6–7–3–3–3–4–6–4–4 in the order in which they are given in the Smithsonian Constants of Nature, 1897, pp. 127 to 131.

The total aggregates 56 determinations for the 12 series; that is an average of 4⅔ to the series.

The highest individual number of determinations is 9; but this should have been counted as two series, of 6 and 3 determinations.

Without going beyond this point, we must therefore condemn as scientific frauds all the probable errors given in the Smithsonian work specified, because the method of calculation is applied in all these cases under an insufficiency of the number of determinations made.

That the probable error is calculated to three and four decimals aggravates the scientific fraud many fold.

Condition by Probability.

In the second place, every one entitled to use this method of the calculation of the probable error is required to know that *the actual differences* have to be distributed according to the law of probability, and symmetric to each side of the mean.

This condition is nearly always violated in the applications made in calculating the probable error of the mean values of the atomic weight determined by any one process in any one series. In fact, no chemist seems to be aware of this limitation.

I have pointed out one of the most flagrant cases of the multitude committed by Professor Ostwald of Leipzig under the highest pretentions to scientific precision. See my "True Atomic Weights," pp. 43-44; 1894.

In the entire big book of the "Constants of Nature," issued in 1897, by the Smithsonian Institution, there is not half a dozen cases among the hundreds given, in which this essential condition has not been violated.

Conditions were Disregarded.

Accordingly, practically speaking, all the calculated values of the *probable error* of the *mean values* of atomic weight determinations published up to-date in all scientific works on atomic weights issued, have been obtained in total disregard of these two fundamental conditions which are pre-requisite to the application of this mode of calculation.

In the determinations of the atomic weight of boron made in the laboratory of Moissan at Paris, recommended by him and his section of chemistry to the Academy of Sciences for a prize which was granted in December, 1900, the study of the probable error was specially accentuated; and yet neither the chemist Moissan, nor his endorsing colleagues know the formula for the calculation of that probable error, having omitted the coefficient $\frac{2}{3}$; all pretended values given and studied (discuté) and crowned by the Academy are *only* 50 per cent too high. See Annales de Chimie, etc., T. 18, p. 363; 1899, where the formula used is printed as "*formule connu.*"

Conditions Applied by us.

Let us apply these conditions to the calculation of the probable error of the mean weight of the twenty silver dollars weighed by us on April 4, 1901. See page 13.

The number of determinations, being 20, is just passable.

The differences are quite evenly distributed about the mean, though it must be noted that the difference for the lightest coin is excessive.

The probable error of 3 centigrammes would therefore be accepted as reasonably well established.

But even this fairly authorized probable error possesses no practical value in this question, the determination of the weight of a silver dollar.

In the record of the ten series of weighings (p. 7) we find the probable errors to range from 2.6 to 4.8 centigrammes, while the mean weights actually run from 26.28 to 26.51, that is over 23 centigrammes, and while all these means are notoriously below the true weight on account of abrasion.

Let us check this case by the condition of an *even wager*, calculating the corresponding probable error of a single determination or silver dollar.

The total number being 20, the square root of which is 4.47 (for which we can take 4½) we shall obtain the probable error of a single dollar by multiplying the probable error of the mean 3 by this number 4½. We obtain 14 centigrammes or 0.14 grammes.

Counting, on the list above given (p. 13) the number of dollars weighing between 14 centigrammes less and more than the mean of 26.51, that is, between 26.37 and 26.65, we find eleven, instead of exactly half the total number. Since it so happens that the weight 26.65 occurs twice, we are perfectly satisfied as to the distribution of these silver dollars according to the law of probability, at least on this most essential condition, so readily tested.

All Published Probable Errors are False.

But this test of applicability being unknown to chemists, they have never applied it in their calculations. If applied, it would condemn almost all the calculations of the probable error made by chemists.

From whatever side we view the probable error of the mean calculated by chemists, we must condemn it as obtained in absolute ignorance of the conditions imposed by science. Hence the results are not only worthless, but they are false and fraudulent.

The Double Distilled Fraud.

It follows without saying that all estimates of the scientific value of series of atomic weight determinations dependent upon the minuteness of this so-called probable error are not only double distilled frauds, but the impertinent, arrogant imposition of an ignorant mechanical calculator who blindly applies a mathematical method he does not understand to the work of chemists he does not comprehend. We refer to the author of the Constants of Nature, which are neither constant nor of nature.

I shall not go into further particulars at this point by giving striking instances of such absurd judgements pronounced on work done by American Chemists, but shall point out a few glaring instances as we go along in the summary of the atomic weight determinations made during the nineteenth century.

The Law of Probability.

As to the *Law of Probability* here referred to, I may be permitted to state, that the same has been independently established by me in a strictly experimental manner as published in my "School Laboratory of Physical Science, vol. II, pp. 28-38; Iowa City, 1872," and also in my "Rainfall Laws, reduced from Twenty Years of Observarion," pp. 43-56, Washington, D. C., Weather Bureau, 1893.

Fully half a million experiments were made by my students. These experiments completely established my simple and practical, graphic method of applying the probability curve, which otherwise had only been accessible by means of difficult methods of higher mathematics.

This remark is here appended to prevent improper inferences and not unlikely insinuations.

All Dice are Loaded.

How sensitive some very common operations are to minute influences readily overlooked by us, we may see in the throwing of dice.

A series of 26,602 casts of 12 dice each (Prof. Weldon's experiment) is lately reported by Professor Karl Pearson of University College, London, in the Philosophical Magazine, vol. 50, p. 168; July, 1900. The total number of "fives" and "sixes" thrown was 106,602. Thus the *fact.*

The total number of dice thrown was 12 times 26,602 or 315,672; hence the *actual* ratio for the fives and sixes was 106,602 divided by 315,672 which is 0.3377.

But the fives and sixes mark one third of the six faces of dice, and should therefore have occurred one third, that is 0.3333 times of all, for strictly even chances.

The fives and sixes actually thrown exceed their theoretical probability (of the even chance) by 0.0044, that is by 44 on 10,000. This corresponds to our "*analytical excess*" in the following.

False Science from False Facts and False Tools.

Suppose one of our modern "exact scientists" proceeds to establish the law of probability by throwing of dice, and takes this *mere fact* of 0.3377 as the *true* probability, would he not get up some very fine science?

He would, in that case, overlook the fundamental error involved in the fact that dice, marked as they are, cannot give a strictly even chance, but necessarily favor the high throws, that is the fives and sixes.

Why? Under the five depressions we have only two, under the six small holes only one; in other words, *the best of dice*, by the method of making, are lightened at the faces with five and six depressions in comparison to the opposite faces, which thus are relatively "*loaded*" because a mere trifle of substance less has been removed.

Nature can not be Suppressed.

Now, the force of gravity cannot be suppressed—it points out with unerring hand this trifling amount of matter. So nature always points out what exact scientists overlook.

And thus we would "*falsely*" condemn the true probability of an even chance if we tried to prove an abstract principle by a mere experiment — in which we overlook one missing condition, the lack of absolute equality in fact.

This error of common dice came to our knowledge when making experiments on the law of probability more than thirty years ago.

The Exact Scientist should be Tested First.

When we try to test *Nature*, let us not forget to test *ourselves* and our tools *first*.

When making our extended experiments on the laws of probability thirty years ago, we found it impossible to get exactly equal balls for the urn from which the draws were made; we did, however, not ascribe these errors to the laws of probability, but to the imperfections of our own means at hand.

The Greatest False Scientist.

If modern chemists did not suppose Stas perfect, the atomic weights of modern chemistry would not present the mysterious muddle they do.

Our modern chemists, under the leadership of Stas, have corrupted chemical science by their assumption of a perfection and exactness in experimentation that existed necessarily only in their own imagination; as a result, the atomic weights actually in use for years are all false, contrary to nature, as we shall prove beyond the possibility of a doubt.

IV. THE CONSTANT ERROR OF THE MEAN.

But if we cannot use the mean of a large number of simple weighings of actual coins in circulation as the true value of such a coin, how dare we assume that the mean value of a very few determinations of the atomic weight of a chemical element will give us its true value, or that we shall approach it more closely by taking the mean of the mean values of a few series of such determinations?

The unavoidable errors in the different chemical processes made use of in these determinations are much more difficult to estimate and comprehend than the mere abrasion, influenced by more or less rapid circulation and by peculiarities in the frequency of certain coinage years over others; but they are not less real than errors due to abrasion.

The unavoidable errors affecting the different chemical processes are, however, not all working one way, as it is necessarily in the abrasion of a coin making actual weight in circulation always lower than its original and true legal weight.

Lower and Higher Limits.

Indeed, for many chemical processes we have means of knowing whether they are giving the atomic weight too high or too low. See Rule of Berzelius, p. 3.

Accordingly, in such cases we know whether we obtain a higher or a lower limit of the atomic weight found by the chemical operation employed.

This will enable us to fix a higher and a lower limit of the atomic weight sought, but in no case is the exact or absolute atomic weight thus determined; for we have no chemical means of ascertaining the exact amount of the excess or deficiency due to the chemical operations used.

The special example here considered, namely the experimental determination of the true weight of a given coin by the process of actually weighing the coin, at hand, shows in a striking manner the insufficiency of mere empirical or experimental work in the determination of any given quantity actually occuring in nature or commerce.

Laboratory Work Alone not Enough.

The determination of the atomic weight of a chemical element being a much more complicated process, involving not only weighings but also chemical operations that bring the material operated upon into chemical circulation, will now be understood to require something more than mere laboratory work and weighings, and even much more than the calculation of the probable error of their mean value.

approaches the year in which the weighings are actually made.

Even in the case of coins, we remain ignorant of the exact conditions of this error, depending on the rapidity and character of actual circulation.

This is precisely the condition which modern chemistry presents to-day in its record of atomic weight determinations. The form in which these conflicting results of experiment are usually presented fails to convey a proper appreciation of the magnitude of these differences.

As it is most important at the outset of this investigation to have a proper understanding of the actual errors prevailing, we shall give the necessary details for the most valuable element—gold.

V. ACTUAL ERRORS OF THE MEAN.

That the mean of a series of atomic weight determinations is, de facto, affected with quite large constant errors (constant for each series or process used) can be seen by the examination of any actual chemical set of determinations.

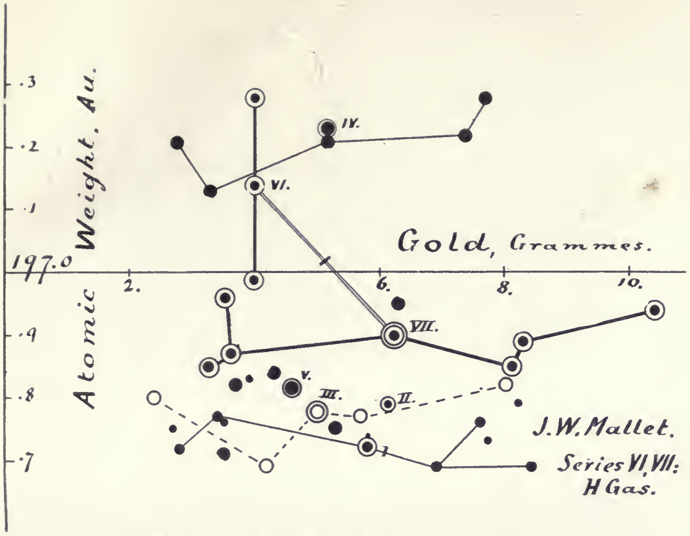
To make the fact convincing, we select the very noted work of Mallet on the atomic weight of gold.

This most noted chemical work on the atomic weight of gold was done by Professor J. W. Mallet of the University of Virginia, and first published in the Philosophical Transactions of the Royal Society (London) for 1889.

Through the personal courtesy of the distinguished author I have been able to study the details of this highly important research from an extra copy of the publication in vol. XII of the American Chemical Journal (Baltimore).

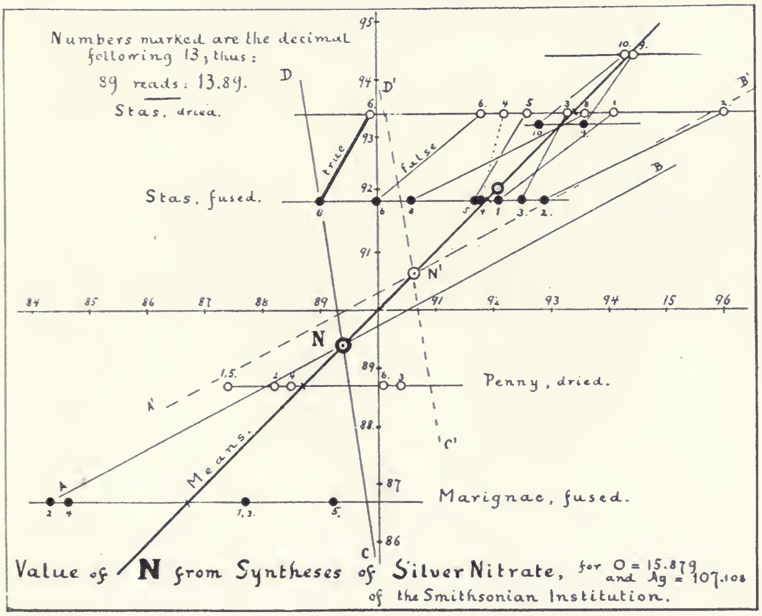
It is almost unnecessary to add that this work of Professor Mallet is justly considered equal to the best chemical work done in this line of research during the last quarter of a century. By fully making this opinion our own we may be permitted to take the results of this chemical research as representing the best of this kind of chemical work now on record.

PLATE I.



MALLET: ATOMIC WEIGHT OF GOLD.

If Mallet's work were true, all dots would lie in a single horizontal line. See pp. 24–28.



STAS: ATOMIC WEIGHT OF NITROGEN.

If Stas' work were true, all dots would be grouped close together. See pp. 186-191.

The Seven Means are all Mean.

Professor Mallet made *seven series* of determinations, according to as many different chemical methods or processes.

This is the common practice of chemical science to-day, and such different series are undertaken in the hope that thereby the errors referred to may balance and thus disappear from the final result, at which the *mean value* of the means of each series is taken.

Exactly why such seven errors should balance and mutually destroy one another so as to give a final mean without error, is not stated.

The means given by Professor Mallet for each of his seven series, and his final mean, are as follows (according to Ostwald, Zeitschrift, IV, 478; 1889):

Series	I,	from	gold chloride, ... Mean,	196.722
“	II,	“	gold bromide, ...	196.790
“	III,	“	potassium gold-chloride, . . .	196.775
“	IV,	“	trimethyl ammonium gold chloride,	197.225
“	V,	“	potassium gold cyanide, ...	196.825
“	VI,	“	same to hydrogen, ...	197.137
“	VII,	“	same to zinc, ...	196.897
			General Mean, ...	196.80

While there thus is a *range* of almost half a unit between the lowest mean 196.775 (III Series) and the highest mean 197.225 (IV Series), the results of each series and of each individual determination are given to the third decimal. It would not look like exact science if less than three decimals were used.

As the most probable mean of the means of the seven series, Professor Mallet gives 196.80 as the atomic weight of gold according to his great chemical research which has been very highly honored.

But we really do not care for the most probable mean value at all. What we would like to know is the *true* atomic weight of gold. If there is exact science, it ought to be able to give us exactly that answer in *one* exact number.

Our Little Diagram Shows the Facts.

But the range of the *individual determinations* in the seven series necessarily show still greater divergencies than the mean values of these series. In order to present these variations to the eye *as they really are* so as to obtain a proper conception of the magnitude of these variations, I here reprint the diagram from page 193 of my True Atomic Weights published in 1894. See Plate I.

In this diagram the actual values given by Professor Mallet himself are entered as *dots* representing also the amount of gold used in the particular determination.

The scale is marked on the horizontal line for the *gold used* (in grammes), and on the vertical line for the *atomic weight found*. The dots representing the actual determinations of any one series are marked in the same manner, and connected by a line.

The mean value of each series is also entered by a dot surrounded by a circle, and marked by the roman numeral of that series. In this way the diagram here given represents graphically and according to scale all the most essential results of the chemical work of Professor Mallet, as given by himself.

It is seen, that neither the individual determinations, nor the mean values of the series, show any tendency to cluster near any particular point.

Mallet did not Hit the Mark.

If the field represented in our diagram were considered as a target and Professor Mallet were to use seven different rifles to hit a definite point on that target, and the marks made in the target were connected by lines for each rifle, we would in that diagram see the evidence that the seven rifles were equally bad, and the shooting so scattering as to show no center hit indicated or marked by any grouping of dots.

In fact, the determinations made by Mallet scatter greatly and cover the entire field on either side of 197 quite evenly, extending fully three tenths below and as much above this line.

If we don't know the Tenth, we don't know the Thousandth.

That is, as a matter of fact, the atomic weight of gold found by the chemical determinations of Mallet ranges about equally on both sides of 197 to the extent of three tenths, making the entire range or uncertainty fully six tenths. Now when the best chemical research made leaves the atomic weight of gold uncertain to the extent of the range of *six tenths* of the unit employed, we are justified in considering the hundreds and the thousandths displayed in this research as quantities utterly unknown, so far as actual chemical determination is concerned.

Surely, the second and even more so, the third decimal in all of these determinations of Professor Mallet cannot for a moment be taken as chemical facts determined by chemical laboratory work, but they are evidently not experimental scientific data at all.

They are extra-polations made by carrying arithmetical calculations beyond the reach of the degree of accuracy of the experimental work done.

Mallet Suffers from Morbus Stasii.

Furthermore, this diagram, representing to scale the actual values given by Professor Mallet himself in the American Chemical Journal, shows plainly that the atomic weight found varied considerably with the amount of gold actually used in the particular chemical determination.

In Series IV, VII and III, the individual dots form approximately a line rising as the amount of gold increases; in Series VI the rise is seen to be perpendicular. In Series I the values tend downward as the amount of gold increases.

But it is the first condition of value of any series of chemical analysis of any one given substance, that the final result must be independent of the amount of material used in the analysis.

That is, the per cent found must be the same, whether 2 or 4 or more grammes of the material analyzed has been employed. Of course, no two analysis will give the same number exactly; but the variations must lie on either side of a horizontal line in a diagram of the kind here used.

If a chemist engaged by a firm to analyze a limestone should hand in a report showing clearly that by his method of analysis he had found the percentage of lime to run up (or down) with the amount of limestone operated upon by him, the firm would most likely not entrust any further work to this chemist.

We regret to find by these symptoms, that Professor Mallet has suffered from Morbus Stasii for many years. This should be remembered before judging his work harshly.

This is so much the more called for as we notice his case complicated by the incipient stages of Furor Clarkii.

Our Conclusion.

Therefore, the only legitimate conclusion that can be drawn from the results given by Professor Mallet and represented graphically to scale in our diagram, is that *the methods used are not chemically satisfactory,* because they give results showing the existence of errors varying with the amount of matter operated upon.

Instead of taking the mean of each series as the true value, *the entire work should be discarded as unreliable.*

We have purposely dwelt upon these details of the noted determinations of the atomic weight of gold by Professor Mallet because the recognized high value of the chemical work shows strikingly the necessity of some critical examinations of such work by methods not yet used in the chemical laboratory, but which can be drawn from the general science of quantity and form, that is, from mathematics and common sense.

VI. ERRORS IN PRECISION.

Incredible as it may seem to any one not familiar with the causes that have led to the existing practice of calculation among chemists engaged in the determination of the atomic weights of the elements, we find, as a matter of fact, that two errors of method are quite generally committed, either one of which would suffice to vitiate the results proclaimed as the atomic weight determined often by great skill and patience in the chemical laboratory.

The Two Fatal Common Errors.

First, the chemist will calculate the atomic weight with two or even more decimals, when a simple examination would have convinced him that these decimals have absolutely no value whatever, being far beyond the limit of precision of his chemical work.

Second, the chemist will start in this calculation with the data of one of the national leaders of organizations, and if these data are wrong (as we shall find them to be) he will necessarily get erroneous values from the very best laboratory work he can produce.

It is very unpleasant to make such sweeping statements; but it is not our fault that these statements are an exact representation of the actual facts.

And if the reader for a moment will free his mind from the power of authority whether it be of numbers, name or station, he will see the necessity of the full recognition of this state of facts in order to clear the way to obtain true results from good chemical work.

Don't give us your Fancy for Fact.

First, we must demand that no chemist publish to the world an atomic weight as representing his laboratory work, his chemical research of precision, when, as a matter of fact, he gives decimals that have no foundation whatever in his chemical determination and weighings, but simply are products of his own imagination.

That which is common practice in the Laboratory of Moissan of Paris, presented by him to the Academy of Sciences of Paris, and recommended by the present Section of Chemistry of that Academy for the Prize Vaillant which was granted by the Academy in December last, cannot be a special sin when committed in an American Laboratory and published in our Journals of Chemistry. I refer to the work of Henry Gautier on boron, in the Laboratory of Moissan.

To state as experimentally established, as data of laboratory work, numbers and decimals that are merely and absolutely fancy, is about as disgusting and reprehensible an act as can be committed against the true progress of science.

Edgar F. Smith and W. L. Hardin.

Still I shall here take the special example for illustration from the laboratory of Edgar F. Smith of the University of Pennsylvania, because it was published in the Thesis of Willet Lepley Hardin in 1896, and in various Journals, after the error had been fully shown by me in letters to Professor Smith in May, 1895.

We take then, as special case for the study of the first great common error in calculation of atomic weight determinations, the six electrolyses of mercuric oxide made by W. L. Hardin in the laboratory of Edgar F. Smith.

The mercuric oxide taken and the mercury obtained are the weighed quantities. The weighings are given to the hundredth of a milligramme (by oscillation method).

Accordingly, the hundredth of the milligramme being the last figure determined, is simply the nearest full number of that place, and subject to the usual limitation of a possible error of at most half a unit. In other words, the utmost that can be claimed is that the weighings given are subject to an uncertainty of half a hundredth of a milligramme, or half a unit in the fifth place of the gramme.

For each of his six determinations Mr. Hardin calculates the corresponding atomic weight, and gives the results with two decimals (Thesis, p. 23). The weighings and the calculated atomic weights are identically the same communicated to me in May, 1895, by the courtesy of Prof. Smith.

But the weighings *do not* sustain any such atomic weights, and the publication of these atomic weights as experimental data is false in fact and fraudulent in nature.

Taking Hg at 200, the oxide exceeds the metal by exactly 8 per cent. Taking the mercury as reported by Hardin, and adding 8 per cent thereto, we obtain *exactly* the weight of the mercuric oxide as determined by him to the exact hundredth of a milligramme.

This is as perfectly and absolutely the case that this series of determinations, if true, is most marvelous and created a suspicion of being manufactured.

In the reduction by Mr. Hardin the atomic weight of mercury is affected with from 0 to 6 hundredths above 200. These atomic weights are published as the expression of the weighings, and are so taken by the chemical public.

Such data as these are not data of observation, for they are not representing the stated weighings; they represent imagination and not observed facts.

It is true, Mr. Hardin has said this series is vitiated by other experiments made with larger amounts; but these six determinations still stand on record as data of actual experimental determination.

If we are to have true atomic weights, we must first blot out all false statements of fact, all invented atomic weights, from the records and publications.

It is well known, that such calculation of decimals can not be carried beyond the limit of precision. Every manual on experimentation gives rules for such limitation, as may be seen in Kohlrausch.

My method is perhaps the simplest, equally applicable in all cases. It is found in my *Elements of Physics*, p. 12; 1870.

It consists in actually calculating the value sought from the formula by using the actual data determined and also modified by one unit in the last place. The difference between the two values evidently is the variation for that unit.

In the case before us, we must bear in mind, that for a given amount of oxide, the increase in mercury would necessitate a decrease in oxygen given by difference.

Accordingly, for the first determination, in which 262.23 milligrammes of the oxide gave 242.81 mgr. mercury—and by difference 19.42 mgr. oxygen, we calculate Hg both for these weights and for 242.82 and 19.41. We find a change in Hg of 0.11.

Accordingly, as *half* a hundredth of a milligramme is the uncertainty of weighing the corresponding uncertainty in the atomic weight is 0.055 or practically 0.06 units in Hg.

But the greatest decimal given by Hardin is precisely 0.06. The determinations of Hardin are uncertain to this extent. Hence these fractions of Hardin are not experimentally determined. They represent nothing but his own imagination.

For the fifth determination the change per unit (in hundredth mgr.) amounts to 0.10; hence the uncertainty in the value of Hg is 0.05. The fraction 0.03 of Hardin is within this limit of uncertainty. His statement Hg = 200.03 is not a statement of fact, is not warranted by his weighings. He is drawing on his own imagination.

For the fourth determination, in which the weight of the oxide taken is stated to be 141.48 mgr. the change (per unit in hundredth milligramme) is 0.20; hence the uncertainty in the atomic weight Hg amounts to 0.10.

Now in this instance, Mr. Hardin gives Hg = 200.00; *both* of these decimals are due to his own imagination, since his weighings leave the atomic weight uncertain to the unit in the first place.

In other words, in the stated value Hg = 200.00 Mr. Hardin assumes for his determinations an accuracy one hundred times the actual precision of his own weighings!

Such statements, pretending to be statements of experimental facts, are a blot upon science and block the way of the truth; for they are false in fact, merely imagination and fancy. They are essentially fraudulent, for they do not present fact as they pretend to do.

VII. ERRORS DUE TO FALSE DATA.

The *second* mathematical error universally committed by the chemists of the present is in itself fatal to the production of a true value of the atomic weight or even a correct expression of the often admirable chemical laboratory work done.

We refer to the common habit of "adopting" some set of values of atomic weights in the calculation required, without making sure that these auxiliary atomic weights are themselves correct.

It is incredible, but true, that chemists will reduce their often excellent laboratory work by means of auxiliary atomic weights furnished by committees of chemical societies or some official chemist without allowing for the possible errors of these auxiliary data.

Necessarily, the errors of these data of the auxiliary atomic weights adopted, will affect the results calculated from the new chemical determinations.

Ramsay and Aston.

The opening words of the otherwise admirable paper of Ramsay and Aston on the atomic weight of boron are:

"The atomic weights employed in this paper are those "given by Clarke: Ag = 107.92, Na = 23.05, Cl = 35.45, "Br = 79.95, H = 1.008 and O = 16." See Journal Chemical Society, vol. 63, p. 215; London, 1893.

If now each and every one of these five values in reference to the oxygen standard be affected by errors, these errors will necessarily affect all the calculated values of Ramsay and Aston.

Let us suppose for a moment, that these excellent chemists had completed all their chemical work with absolute precision and therefore free from experimental error, the results published would still be erroneous to the extent of the effect of the errors of these five atomic weights of Clarke "employed" by them.

We shall, in another section, show conclusively that these chemists thus falsified their own most excellent laboratory work.

We may already at this point call attention to the fact that these authors find the atomic weight of boron 10.965 by calculating from sodium chloride, while calculating from silver chloride they find it 11.084. See determinations Nos. 22 to 26.

As the atomic weight of chlorine appears in both of these compounds, the very large discrepancy of 0.119 is connected with the Clarkian atomic weights for Na and Ag "employed." This amounts to *one per cent* on the atomic weight—an enormous error!

It is passing strange that good chemists continue to reduce their weighings by such data and coolly recognize the discrepancy of more than one-tenth in final means though they have been giving single determinations to the thousandth.

Have such chemists never felt the necessity of inquiring into the cause of such discrepancies, exceeding hundredfold the supposed accuracy of their own chemical work?

H. Moissan and H. Gautier.

The atomic weight of boron has also more recently been determined in the laboratory of the University of Paris, under the direction of Moissan, by Henri Gautier, which determination has been greatly honored by the Academy of Sciences of Paris, granting the Vaillant Prize to the young chemist upon the recommendation of the entire section of chemistry, for which section Moissan was the spokesman (rapporteur).

In the reduction of his often admirable laboratory work, Henry Gautier uses "the table of atomic weights published in 1898 by Landolt, Ostwald and Seubert" throughout his reductions.

Consequently, he will have all his chemical determinations *infected* by the errors of these atomic weights of the German Chemical Society.

Under Moissan, good French laboratory work is spoilt or falsified, by reducing it by German atomic weights.

That such is the case I have shown in my two articles communicated to the Academy in the sessions of June 18 and July 2, 1900, which were published in full in the Comptes Rendus.

That a great Academy of Science grants a valuable prize for work which has been shown in its own publications to give false values is a rather important fact to take note of.

When we come to the study of the atomic weight of boron we shall give ample data on these astounding and most characteristic points.

There is not a shadow of doubt but the values crowned by the Academy of Sciences of Paris are not true to nature, and this fact was fully established by my papers printed in the Comptes Rendus of that academy six months before conferring that prize.

Armand Gautier and J. Aloy.

To what extent such erroneous data (auxiliary atomic weights) will falsify the results of the chemical work, we will show by one single example.

This case we shall also take from one of the highest sources of chemical science, from the noted laboratory of Professor Armand Gautier, of the University of Paris, where Mr. J. Aloy has made the research, the results of which Professor Armand Gautier himself presented to the Academy of Science on March 4, 1901 (C. R. 132, p. 551–553).

In this research the atomic weight of Uranium is determined by a process of ignition and combustion, the nitrate giving free nitrogen gas and uranic oxide. The nitrogen is measured, the oxide is weighed.

No data of weights are given. The volume of nitrogen, and the atomic weight of Uranium resulting are stated.

This is a very improper way; the direct data of the quantities determined must be given to admit the research to the records for any use whatsoever.

No critical examination is possible in the absence of the statement of the real weights determined. Moissan, in his determinations of the atomic weight of fluorine has also omitted these essential data; hence his determinations could not be introduced into the general record. See True Atomic Weights, 1894, p. 195.

It is to be hoped that this singular practice of Moissan and Gautier, will not become general in France. It would certainly blot out French work from the record.

In the absence of the necessary weights of the two quantities determined in each experiment, we are compelled to test the results given by an inverse process.

The atomic weight in reference to nitrogen is given, and this atomic weight of nitrogen "is taken at 14.04."

The atomic weight of uranium stated ranges from 239.3 to 239.6 occuring each once. Two determinations gave 239.5 and four gave 239.4. Eight determinations were made.

Accordingly, the analytical ratio Ur : N ranged 17.044 to 17.066.

But if instead of taking the atomic weight of nitrogen as 14.04 it were taken at 14 exactly, the above ratios multiplied by 14 would give for uranium values ranging from 238.6 to 238.9, which are 0.7 lower than the values reported by A. Gautier on the assumption that N is 14.04.

Here we have a change of seven tenths of a unit in the atomic weight of uranium consequent upon the slight change of the atomic weight of nitrogen from 14.04 to 14, amounting to four hundreths only.

All previous determinations make it highly probable that Ur is 240. Gautier reports 239.4 for N = 14.04; for N = 14 it would come down to 238.7.

This shows strikingly the great importance of the values of the auxiliary atomic weights used in the calculations of the chemical experiments made, that is, in the so-called reduction of the work.

The very considerable change or uncertainty also emphasizes the necessity of giving the original data of the determinations, the direct weighings (reduced to vacuum only), so that the chemical work done may be used and not have to be thrown away.

We shall find, in a subsequent part of this work, that the real atomic weight of nitrogen is 14 exactly and not 14.04. The weighings of Lord Rayleigh have made the Stasian value 14.04 absolutely impossible. See my General Chemistry, 1897, p. 378.

Since now the atomic weight of uranium will be found to be truly 240, according to unquestionable methods of work, the new determinations made in this Laboratory of Armand Gautier have only added another false value to the chemical record.

In fact, the work done by Mr. J. Aloy in the laboratory of A. Gautier, has been absolutely thrown away, as utterly worthless.

My Method in the Comptes Rendus.

The necessity of fixed and true values of the auxiliary atomic weights is so palpable that it should never have been overlooked.

That the use of different values of such auxiliary atomic weights in different hands must give different results for the same atomic weight, even if the chemical determinations were exactly the same, should never have been lost sight of.

My general method of keeping an exact account of the effect of any slight change in the atomic weights, was published in the Comptes Rendus for 1893 (T. 116, p. 695–698; March, 1893).

It is deplorable that the famous directors of the great laboratories supported by the French Nation are unable to profit by the contents of the Comptes Rendus of the Academy of Sciences of Paris.

It seems strange that these most eminent Chemists of Paris do not prevent the falsification of excellent chemical determinations of their French students by the use of incorrect German atomic weights.

But if these most eminent Chemists of Paris persist in disregarding the most obvious and elementary principles of mathematics, the work of their students will necessarily be in error, and worthless to Science.

We shall see whether modern chemistry will accept the conferring of a prize by the Academy of Sciences of Paris, at the recommendation of its section of chemistry, sufficient to adopt these palpably false values of the atomic weight of boron as true.

I may be pardoned the expression of the hope, that the great laboratories of Paris will in a near future pay some attention to these methods of reduction.

It is but a few years since Schutzenberger and Friedel have been lost to French Chemistry. It is particularly regretted that A. Gautier has so soon forgotten his colleagues, who in publications and in their rostrum made practical use of my work.

It is not for me to examine how it happens that these leading chemists of France are foisting upon the chemical public of the world, work crowned at their recommendation by the famous Academy of Sciences of Paris, work that gives new data notoriously and necessarily erroneous, because these great chemists have not found time to intelligently read the papers published in the Comptes Rendus of their own academy.

So far as Professor A. Gautier is concerned, he has repeatedly acknowledged the value of my contributions to the Comptes Rendus and the importance of my critical examination of the work of Stas, which implies the falsity of the value of 14.04 for nitrogen.

Is this throwing away the labor of young French Chemists and the production of new errors and false atomic weights in any way connected with that old French Institution: Le Cumul?

VIII. ERRORS OF WEIGHING.

The last laboratory operation in a chemical determination of the atomic weight is the *weighing* of the *product* obtained.

The first operation, after obtaining the pure material or *substance* was likewise a weighing, namely the determination of the exact amount of substance operated upon.

These weighings may be reduced to vacuum.

The weight s of the substance taken, and the weight p of the final product obtained, are the only data of the actual experimental determination made.

To express these results in a common unit, *all experiments are referred to the unit of weight,* by dividing the weight of the product by that of the substance used.

The quotient thus obtained is the only true final expression of the experiment or determination made. We call this quantity the *analytical ratio.*

Every individual determination actually made is thus expressed by one single number, this analytical ratio, a, which is the amount of product obtained for the *unit of weight* of substance.

This ratio is, as stated above in words: $a = \frac{p}{s}$.

In order that there may be no misunderstanding about this most important — though really very simple — matter, we will take from the record one of the most noted cases, namely the five combustions of diamond in oxygen by Dumas in 1840.

We copy the data from the original publication, Annales de Chimie et de Physique, 3rd series, Tome I, for January, 1841. It is reprinted in the "Oeuvres" of Stas, vol. I, pp. 235–287; Brussels, 1894. See also our True Atomic Weights, pp. 20–22, 1894.

Dumas. Combustion of the Diamond.

Exp.	Diamond. Number.	Diamond. Weight.	Ashes.	Pure Carbon.	Carbon Dioxide.	Analytical Ratio.
1	Scales	717	9	708	2598	3.66 95
2	18	865	1	864	3167.5	3.66 61
3	6	1221	2	1219	4465	3.66 28
4	5	1233	1	1232	4517	3.66 64
5	Grains	1377	2	1375	5041	3.66 62
All weights in Milligrammes.					Mean	3.66 63

This is all we learn from the five famous combustions of diamond made by Dumas in 1840, and in which Stas was privileged to assist.

The five combustions are each represented by the final analytical ratios (product obtained divided by substance used).

These ratios represent the weight of carbon dioxide obtained by the combustion of a unit of weight of the pure carbon of the diamond.

These five ratios agree so closely that we are authorized to calculate the mean value of all.

But we must not conclude that this mean value 3.66 63 more nearly represents the true value than either one of these individual ratios.

We have sufficiently explained this point and shall have to come back to it when we reach the discussion of the atomic weight of carbon in relation to that of oxygen.

Precision of Weighing.

At this point we shall now consider the accuracy of weighing and the degree of certainty of the ratios calculated, that is, the number of decimals that are reliable.

First, as to the accuracy of weighing, we know that it generally exceeds the accuracy of the chemical operations and processes involved in changing the substance taken into the final product.

It is therefore, on the whole, to be carefully born in mind, that the accuracy of weighing exceeding that of the chemical processes, the accuracy of the final weights is less than the accuracy of the actual weighings, and especially that the ratio calculated is subject to even an uncertainty due to the most accurate part of this work, namely, the weighing.

It is of the highest importance that this subject should be fully understood in order that the data of experience obtained in the chemical laboratory can be taken for what they really are — neither less nor more accurate than the actual work done.

We therefore shall consider the points mentioned separately, one at a time, with such detail as seems necessary.

The Balance used by Dumas.

The weighings above given are expressed in milligrammes; only in one single instance is the half milligramme stated. This does not mean that this half milligramme was actually weighted by a weight, but 7 milligrammes were plainly insufficient and 8 milligrammes as much in excess.

This little circumstance of the single half-milligramme recorded shows that the weighings were exact to the nearest milligramme, and no more.

Dumas himself states that the "balance used very readily shows the milligramme" (Oeuvres, p. 251). He mentions this fact incidentally, when stating that the diamond burnt surely did not contain hydrogen—not enough to give one single milligramme of water from the combustion of a diamond weighing 1,500 milligrammes.

As will readily be understood, this shows that a diamond of 13,500 milligrammes cannot contain as much as one milligramme of hydrogen.

Considered with reference to the analytical balances now in the laboratories, the balance used by Dumas in this great research was but an inferior instrument.

This shows once again, that the accuracy of the instrument at hand counts for very little in the value of real scientific work done.

Indeed I am tempted to say, that the very fine balances now in our laboratories, are *one* cause of the inferior work done in these laboratories the last forty years.

The Balance used by Berzelius.

If now we turn further back in the history of this great determination of the atomic weights of the elements to the real founder of this work, Berzelius, we find that the balance used by him during the first twenty years was much inferior to the balance of Dumas of 1840.

For we find, in the earliest data of Berzelius for lead, that he ordinarily took ten grammes of lead and gave the weight of the product to the centigramme only.

Quite a number of the actual weighings of Berzelius are given on pages 13 and 15 of our True Atomic Weights, 1894. He gives generally the centigramme only; at times he states the half centigramme by entering 5 as third decimal.

We are therefore certain, that Berzelius, about 1810, weighed habitually to the centigramme only, while Dumas at 1840 weighed to the milligramme.

Our Fine Balances.

As a matter of fact, the best work done to-day in our chemical laboratories is not reliable below the tenth of a milligramme.

But the fine analytical balances in actual use in our laboratories permit, by the so-called method of oscillations, to *calculate* the weight to the *hundredth* of the milligramme.

Yea, the weighings of Morley are stated to the thousandth of the milligramme, and those of Ramsay and Aston before referred to are stated to the ten-thousandth of the milligramme.

To the uniniated it may appear as prima-facie evident, that the apparent weighings being
of Berzelius to the centigramme or 2nd;
of Dumas to the milligramme or 3rd;
of the present to the tenth mgr. or 4th;
of E. F. Smith to the hundredth mgr. or 5th;
of Morley to the thousandth mgr. or 6th;
of Ramsay to the ten thousandth mgr. or 7th
decimal of the gramme, the work of Ramsay was one hundred thousand times as accurate as that of Berzelius.

Let us see about the facts; for we can not afford to take sham accuracy for the truth.

Weighing the Weighers.

Now both Berzelius, in 1826, and Ramsay and Aston, in 1893, determined the amount of water of crystallization in borax.

Berzelius in three determinations found 47.10 per cent; that is, his analytical ratio was 0.4710 being the amount per unit of weight.

Ramsay and Aston found a mean ratio of 0.471677. Apparently they determined this ratio to the millionth, while Berzelius reached the hundred thousandth only.

But when we examine the *seven* individual determinations made by Ramsey, we find they run all the way from 0.471099 as the lowest to 0.472026 as the highest. See l. c., Journal Chem. Soc.

But the variation thus actually affects the *third* decimal, which being uncertain, all the rest from the fourth to the sixth are — well, good for nothing as experimental data.

In other words, the chemical work of Berzelius, probably effected by means of a good centigramme balance, is fully as accurate, chemically speaking, as the most pretentious weighings on record made to the ten-millionth of the gramme, by Ramsay and Aston in 1893, using one of the finest balances ever made and supplying the fine weights by calculating from the oscillations of the pointer over the scale.

We shall learn, after a little, that Berzelius really got nearer the truth with his centigramme balance than did Ramsay with his balance one hundred thousand times more sensitive (at least on the record printed) than the balance of Berzelius.

Great Chemist, Poor Balance.

If we were given to calculating the "weight" of the work done by chemists, as is customary at Washington, what would be the comparative value of these chemists?

The "weight" of determinations varying inversely as the "square" of the "errors," it would follow that *Berzelius* was in 1826, a 10,000,000,000 times better chemist as *Ramsay* in 1893.

But it is well understood that we do not indulge in such calculations. They belong to the scientific departments at Washington. However, this result remains: Berzelius came nearer the truth with his simple means than did Ramsay with all the refinement of modern science.

sarcasm ?

What then is the real chemical lesson which we should learn from this remarkable incident put on record in all works on atomic weight determinations—though, perhaps, not brought out quite as strikingly.

The real errors committed by Ramsay and Aston in this work were:

First, giving a fictitious degree of accuracy—at least two decimals too many.

Second, using a balance much too fine for the chemical work to be done.

Third, they did not realize that the finest weighing cannot possibly compensate for the lack of purity of substance or the absence of perfection in the chemical operation.

So long as it is impossible to obtain absolutely pure crystals of borax, having exactly all the theoretical water of crystallization and no more, it is absurd to weigh more accurately than to the milligramme.

The Man and the Balance.

In concluding this most instructive episode from the history of atomic weight determinations, I trust the reader hereafter, when studying some new atomic weight determination and noticing how the accuracy of the balance and weights used is extolled, will not conclude that this guarantees accuracy in the final results.

If the *man* behind the gun tells on the result in battle, the *chemist* before the balance tells on the resulting atomic weight.

What has been said may suffice on the subject of weighing. A few words are still required on the calculation of the *analytical ratio*, especially as to the number of decimals that ought to be retained.

Of course, according to the novice it is only a question of physical endurance and space which limits the number of decimals in the quotient calculated from the two observed weights of product and substance.

Official Rule.

Sometimes it is a *rule* officially given, independent of the case in hand. I am afraid this is quite often the only limit observed.

I vividly recollect how, more than thirty years ago, I noticed the specific gravity of limestone specimens marked to seven decimal places each, without fail.

This was in a great scientific military establishment—supported by Uncle Sam.

Knowing how sensitive military scientists are, I ran my eye over a large number of the samples of buiding stones, corresponding samples of which were known to be in my own hand for investigation, before I dared ask for "more light."

Upon putting my question in the most modest form as a request for information, I was informed that these specific gravities were calculated by means of seven place logarithms.

Seriously and most earnestly I here am compelled to declare, that much of the vaunted high accuracy of the so-called *exact science* of to-day in our laboratories, our publications and our academies of sciences, sciences has no better foundation in nature or fact than had the last four of the seven decimals given in beautiful and distinct writing on the labels of common building stones in one of the military science shops of the great United States of America.

True and Sham Accuracy.

But if we would lift the fog that has settled over the true values of the fundamental data of chemistry, the atomic weights of the elements, since the first publication of the misleading and muddled work of Stas, we must learn to distinguish between true accuracy and sham accuracy.

We must again rely on the chemist and not merely on the balance and the weights.

If Ramsay, with the finest balance oscillating to the ten millionth of the gramme, did not get as good and true a chemical result as did Berzilius eighty years earlier by means of a balance not better than the prescription balance of common American drug stores, we must cease to judge by the apparatus and again demand the work of a true chemist.

The Number of Decimals.

And how shall we limit our number of decimals in the calculation?

Any one can answer this question, both theoretically and practically.

The theoretical answer can be readily given by our general mode of calculation stated before. See pp. 29–32.

The practical answer in this case is the simplest possible. It is useless to give more decimals than one beyond the first varying digit.

In the case of the water of crystallization, the third decimal in the analytical ratio of Ramsay and Aston varied from 1 to 2. Hence only four decimals should have been recorded, instead of six.

Or to avoid misunderstanding, the tenth of per cent of water of crystallization running from one to two, only one decimal more ought to have been given, the hundredth of a per cent—as decidedly uncertain.

A Fine Probable Error.

I cannot help adding, as a fine commentary to the above, the "probable error" calculated by Clarke, our government chief chemist, in his "Constants of Nature," published by our Smithsonian Institution in 1897.

On page 172 the probable error of this result is given as 0.0086 per cent, or let us plain chemists put it at 0.009 per cent or 0.00009 per unit (our ratio).

How easily our government chemist, by a little mechanical calculation converts experimental results uncertain in the third decimal to fine work with a probable error less than a unit in the fourth place!

No wonder that even students in our universities are making atomic weight determinations. Their balances and weights are finer than those Berzelius used—and they can also calculate the "probable error" of their result, something that Berzelius did not do.

And as A. Cornu of Paris (Nov. 16, 1894), wrote me in a letter very commendatory of my "True Atomic Weights,"

> "the extension of the method of the least squares has "unfortunately persuaded many people that systematic errors do not exist any more!"

We have given evidence of the existence and great magnitude of just such errors.

IX. MINUTE CHEMICAL ERRORS.

Having critically considered the common process of reduction of the experimental work of atomic weight determinations, we may next point out the leading chemical features of this work.

This will be necessary because we must obtain some definite knowledge of the general chemical principles involved in this experimental work in order that we may be able to judge of the relative value of the different processes in use.

This somewhat systematic view of the experimental chemical work is also necessary because our general works on chemistry give almost no information on this great subject of the determination of atomic weights.

This chemical work consists in the following operations: I, selection or preparation of the material; II, performing the chemical operation whereby the new material is obtained; and III, determining the weight of these two materials.

It has already been stated, that the material must be absolutely pure, the reaction or process complete and definite, and the weighings accurate.

As only definite chemical compounds can undergo definite chemical reactions, the starting material must be a definite chemical compound or element, and the material resulting from the reaction must also be of such chemical nature.

The necessity of obtaining the weight of these two materials greatly limits the choice of compounds that can be used. Hygroscopic, efflorescent and otherwise readily changeable materials must be excluded; for they cannot be handled and weighed with precision.

It is very true that Stas and many of his imitators have by "skill" fancied to overcome these difficulties; but we shall see, that these attempts were disastrous to chemistry.

This is precisely what Berzelius warned against in his Rule of 1814, printed p. 3. He means to say that the completing of the reaction must depend upon the chemical and physical properties of the materials themselves, and *not* on the skill of the operating chemist.

Unfortunately, our modern chemist tries to show off as a sort of chemical acrobat or virtuoso, able to do something very difficult, while Berzelius and his school skillfully used the properties of matter with the sole aim of obtaining true and reliable results.

The starting material must be an element or simple compound, strictly pure and accurately weighable.

Our Standard of Matter.

The diamond is pre-eminently such a material. It has been selected by us as the *standard of matter* for all atomic weight determinations. Comptes Rendus, T. 117, pp. 1075–1078; 1893. True Atomic Weights, p. 174; 1894.

The properties peculiar to the diamond which make it almost the only substance fit to be taken as standard of matter are its absolute resistance to all ordinary physical and chemical agencies—making it weighable with highest precision; and its practically absolute chemical purity, the foreign matter present being incombustible and remaining as a perfectly fixed, exactly weighable ash at the close of the combustion. See p. 39.

The diamond occurs native in perfectly suitable specimens, of not an excessive cost, considering its almost ideal qualities as the fundamental standard of matter for chemical science.

As the diamond will stand the action of even aqua regia and quite a considerable degree of temperature without change, all accessory accretions may be completely removed.

The atomic weight of the carbon of the pure diamond we take as 12 exactly.

It is most essential to state plainly already at this point, that no form of carbon can be used for this purpose of a standard of matter, other than the diamond. Even graphite cannot be employed, and artificial forms of so-called carbon are entirely out of the question. These forms of carbon all lack one or the other of the properties of the diamond. Already Dumas noticed that they cannot be weighed with accuracy. We shall come back to this subject under carbon.

Pure oxygen we can obtain by proper washing and drying.

The combustion of the pure diamond in pure oxygen gives only carbon dioxide gas which is completely absorbable and accurately weighable, as was first practiced by Dumas in 1840. True Atomic Weights, pp. 19–22; 1894. Also p. 39, supra.

The weight of the *product*, here CO_2, divided by the material used, here the diamond—carbon, C, each one determined by actual weighing, gives us the *analytical ratio*, here 11 to 3 with almost mathematical exactness, as we shall show in a subsequent chapter from the record; also p. 39.

In this process of almost ideal perfection we have one type of excellent atomic weight determination.

As result hereof we find oxygen 16, exactly, as we shall show. Thus practically, we have in $C = 12$ as diamond standard of matter also found the old Berzelian Oxygen Standard at 16 exactly.

Oxidation of Metals.

Some metals can be chemically produced in an almost absolutely pure state, permanent in dry air, hence accurately weighable.

Some of these metals can, at a moderate degree of heat, be completely converted into a definite, fixed oxide, which therefore is also exactly weighable.

Hence such metals are suitable for very accurate atomic weight determinations by such a process of oxidation.

Some of these metals, such as tin, may, by heating in a current of hydrogen, again be *reduced* to the metallic state, and thus permit a double determination of their atomic weight.

These methods of direct *oxidation* and *reduction* are among the best, most direct and most accurate of all methods of atomic weight determinations, and were used and perfected by Berzelius and his disciples during the first quarter of the nineteenth century.

These standard methods of atomic weight determinations we shall find to be infinitely more accurate than many of the new methods.

Dry Way Processes, and Crystals.

In general, all dry way processes are infinitely preferable to wet way processes.

Erdmann and Marchand weighed the mercuric oxide and distilled the mercury over by means of heat, collecting the last trace of the vapor of mercury by a gold leaf. The results obtained by this dry way process are among the very best in the annals of chemistry, as we shall find.

Here the analytical ratio is the weight of the mercury collected, divided by the weight of the oxide taken.

Another class of dry way processes we have in simple ignition or dissociation.

The purity of the material used is generally depending upon the process of crystallization and careful re-crystallization.

Thus pure blue vitriol will leave the fixed black oxide of copper, as practiced by Richards.

Ammonium Alum leaves a fixed residue of Alumina (Mallet).

The remarkable finely crystallized Chloro—and Bromo—Platinates leave upon ignition pure platinum direct or after washing the residue with water according as the Ammonium or Potassium Salt has been used. Most excellent determinations have been made in this line by Seubert and Halberstadt.

The related Potassium Bromo-Aurate has furnished, by Krüss, the most accurate determination of the atomic weight of gold.

The ignition of purest Iceland Spar gave us the most reliable determination of the atomic weight of calcium (Erdmann and Marchand).

We shall find that the ignition of the purest native magnesite (from Frankenstein) has given us, in the determinations of Scheerer, really the true atomic weight of magnesium.

All these dry way processes are simple, direct, complete, and permit accurate weighings. They are necessarily the most reliable, although modern chemists have not estimated these processes properly.

The starting material, the compound used, is often either some native or artificial crystal.

The purity of the starting material thus is dependent upon the crystallizing power.

Crystallized minerals, such as quartz and calcite—and crystallized salts, such as alum and the vitriols, have been known from the earliest times.

These bodies are the first indications of definite chemical compounds, therefore the very foundation stones of chemistry.

The power of crystallization also permits us to obtain chemically pure materials for our atomic weight determinations.

We may safely assert that chemical science depends more on this power of crystallization than on any one other; without this power, the very idea of a chemical compound would perhaps not yet have been acquired.

The native and artificial crystals presented and produced the idea of chemical individuals and compounds first in our mind.

The most marvelous of all crystals, the diamond, we have found to be the most, if not the only suitable standard of matter for all atomic weight determinations.

The crystallized carbides, produced by the electric furnace of Moissan, were pointed out by us in 1894, as most important materials fit for exact work to connect elements directly with carbon. (True Atomic Weights, pp. 175–176; 1894.)

This process has been actually used by Henri Gautier in the Laboratory of Moissan, as will be stated in the chapter on the determination of the atomic weight of boron.

This method is quite general and will become of great value by directly linking the atomic weight of many elements to that of carbon.

It is strange that this method was used by Moissan without mentioning its origin.

X. LARGER CHEMICAL ERRORS.

In modern days we have learnt to produce another class of bodies in almost chemically pure form, namely those bodies which are volatile enough to be distilled.

Even silver has been distilled in the lime retort by the heat of the oxyhydrogen blowpipe for use in atomic weight determinations; but we here refer to much more volatile substances which permit their distillation at a temperature readily controlled.

Such are especially the chlorides and bromides of certain elements not readily subjected to dry way oxidation and reduction.

We refer here to the use of silicon chloride and bromide by Thorpe and Young and the corresponding compounds of boron by Henri Gautier.

The silicon bromide used by Thorpe and Young boiled at the fixed temperature of 153 degrees.

These chlorides react with water, producing the oxide and the corresponding acid.

The insoluble oxide is generally separated in a weighable form, and the acid may be determined by converting it into the weighable silver compound.

This mere statement of the process is sufficient to show that it is necessarily much inferior to the ordinary dry way processes.

As unfortunately great errors have crept into chemistry by the silver process—especially when the chloride is concerned—these apparently very fine methods will be found to be of a secondary value only.

The silver process here referred to is the comparison with weighed amounts of pure silver, not the volumetric process proper, which latter we shall mention further on.

Other wet way processes terminating with dry substances and therefore permitting the actual weighing of the final products, have been introduced during the last quarter of a century.

Good Special Methods.

One of the most interesting and accurate of these processes is the conversion of anhydrous borax into sodium chloride by distillation with muriatic acid and methyl alcohol. Sodium chloride is left and weighed.

It can be objected, that this process is somewhat indirect, since the product does not itself contain the element in question, boron.

But the product is in excellently weighable condition, and contains the metal wherewith boron was combined in the substance, borax.

Hence the value of the process depends entirely on the completeness of the chemical reaction used.

Sometimes it is almost impossible to obtain an exactly weighable substance for the initial material, because of the difficulty of removing definitely all water of crystallization or some other secondary constituent.

If now that compound permits the exact determination of the element sought and some other of which the atomic weight is known, good atomic weight determinations are possible, though the original substance cannot be weighed.

The most valuable application of this method we find in the splendid work of E. Maumené on silver acetate. True Atomic Weights, p. 196; 1894.

He determined the silver and carbon in this compound. The silver as metallic residue, the carbon as dioxide.

We shall find this the only strictly unobjectionable determination of the atomic weight of silver.

The method used for the determination of the atomic weight of uranium in the laboratory of Professor Armand Gautier is of the same general character. In this case, the atomic weight of uranium is expressed in that of nitrogen. See pp. 35-36 above.

Methods Giving Variable Results.

We have not yet referred to the conversion of a weighed amount of a pure metal into a definite salt by means of an acid.

In this way Berzelius produced lead sulphate, and Stas obtained also the nitrate of lead and of silver.

Stas has laid great stress upon these syntheses of silver and lead nitrate. He even challenged the chemists of the world to show that his results were not exact. True Atomic Weights, p. 34.

But his own data show that this method is not applicable to atomic weight determinations for silver and lead.

This has been fully shown in our True Atomic Weights, 1894, and in the Comptes Rendus, T. 116, pp. 431–433; 1893. We shall again demonstrate it in this work, but shall not enter into any detail at this point.

When the silver nitrate produced per unit of weight of the metal quite notably varies with the amount of silver operated upon, and even constantly differs according as the nitrate is "dried" or "fused," the process used is simply not fit for the determination of atomic weights.

Methods of a dubious value are those wet way processes in which a precipitate is separated and weighed. All chemists understand that these methods are not quite exact for atomic weight determinations.

Neither barium sulphate nor silver chloride is absolutely insoluble in the liquid used, nor absolutely free from foreign matter.

Processes of this kind must be expected to give values not quite exact.

False Methods.

Even common acidimetric tests have been applied for atomic weight determinations, by Julius Thomsen of Copenhagen, and Richards of Harvard.

Richards tried to determine the sulphuric acid left after the electrolysis of blue vitriol by this method—and got wonderful results, since he overlooked that a part had changed to persulphuric acid, which has only half the saturating capacity. See True Atomic Weights, pp. 135–136.

Here also must be mentioned the volumetric process of Rimbach (1893) on borax with hydrogen chloride, using methyl orange as indicator.

It is really strange that chemists can so far forget the fundamental requirements of atomic weight determinations as to think of volumetric processes of this kind.

But there is one wet way process which has caused many errors to take deep root in the chemistry of atomic weight determination. Together with the Stasian syntheses of the nitrates, the volumetric silver chloride process has muddled this part of the science for almost forty years.

It will be necessary to say a few words about this process by way of a general protest against its common use.

The silver chloride volumetric process is most valuable in technical analyses, and nothing here to be said is intended to reflect upon the method of *Gay-Lussac* and *Mohr* when restricted to technical problems.

But we must insist that this method is unfit for the work of atomic weight determinations. See True Atomic Weights, pp. 121–128.

In order to keep all the silver in the precipitate, there must be an excess of the chloride. But this solution then reacts again with a drop of silver solution.

If the solution gives no further reaction with silver, it will again react upon the addition of a chloride.

Clearly, the silver chloride precipitate is held down by an excess of the soluble chloride; the amount of silver is in no fixed proportion to the amount of chloride in the liquid.

These facts were fully presented by Mulder, and have been admitted by Stas, who supposed that the *mean* between the silver and chloride limit marks the true compound.

We have no inclination to consider fine spun imagination such as this one of Stas or the apparent "ion"-philosophy of Hoitsema presented in Ostwald's Zeitschrift (XX, 272–282; 1896).

It is sufficient for us to know that the chlorine and the silver are *not* present in fixed, definite proportion in this process, but vary very greatly.

We are tired of being called to facts, when the facts are imaginations and dreams in the head of so-called exact chemists.

Chemists must cease to take the fancies of Stas and his school as facts.

We shall not discuss this point, but insist on the facts.

It was Pelouze, about 1845, who introduced the volumetric silver chloride process into atomic weight determinations.

This ready method was unfortunately used extensively by Dumas up to the time when Stas began his pretentious work.

Louis Henry of Louvain.

How the reputation of the work of Stas is now being kept up, we must show by one of the most notorious examples, which also will show, how miserable the case must be when such methods are resorted to.

The school of Stas has wrapped this part of our science in a dense cloud, and kept the atomic weights of the elements in a muddle.

The great authorities who admired the show of decimals and the system of calculation—aped after mathematical patterns—have continued to point with pride to the "scientific work of Stas," of the Academy of Brussels.

The Academy of Sciences of Brussels in public session (Dec. 17, 1899) listened with admiration to an address by its member Louis Henry of Louvain, who glorified his boyhood fellow townsman and repeated silly criticisms of my True Atomic Weights second hand.

This address was published by the Belgian Academy in elegant form, and circulated with great diligence. Comptes Rendus, T. 130, p. 691.

Upon request I received a copy from the author, Professor Louis Henry, in the summer of 1900.

In his letter of transmission he disclaims all personal knowledge about the branch of chemistry in question. He also states that he had not read my work himself.

Having read his address, so highly applauded by the Academy of Sciences of Brussels, I am personally convinced of the absolute truth of his statement in his letter to me, namely that he does not personally understand the subject of atomic weights he discusses, nor had any personal knowledge of my book which he condemns.

If Louis Henry were simply a professor of chemistry of the Stasian school, I would take such an occurrence as a matter of course.

But Louis Henry is Professor of Chemistry at the Jesuit University of Louvain.

That a Jesuit rushes ignorantly to the defense of the false doctrine of a man like Stas, simply because he and Stas were boys together, is the remarkable feature of this case.

In order to avoid any misunderstanding I beg to add that I have the highest respect for his church and for many men of his order, and remember with especial gratitude the kindness shown my earliest contributions to science in the great works of Father Secchi on the Sun and on the Unity of the Forces of Nature. See my General Chemistry, 1897, p. 29, p. 239.

PART SECOND.

The Absolute Atomic Weight of Ten Leading Elements.

I. OUR METHOD OF DETERMINATION.

Having briefly presented the mathematical and chemical methods equally required in the making of any atomic weight determination, and having indicated some of the common errors committed in work of this kind, we may begin the exposition of our own method by which we have obtained the absolute values of the atomic weights of the chemical elements.

It has been demonstrated that the common habit of arbitrarily "adopting" some set of atomic weights in the reduction of new determinations is not only absolutely irrational in theory, but leads to gross errors in practice.

While chemists have been calculating their new work to the second and third decimal, they have, by the above practice, started out with errors ten and even a hundred times as large. See pp. 33–37.

Absolutely Fixed Points Needed.

As in triangulation and even in common leveling, perfectly well marked starting or base points are required and carefully made often at great labor and expense, so *we must in this fundamental work in chemistry use certain absolutely fixed data in all our calculations,* in order to avoid the introduction of errors by the process of calculation or reduction.

Now such data we have in the indisputable fact that the atomic weights of all elements are quite near whole numbers, if we take oxygen as 16 exactly.

In a few cases, as for copper and chlorine, the atomic weight approaches the exact half unit.

Moreover these atomic weights have always been extensively used under the name of *common atomic weights.*

The *true atomic weights* of the elements are experimentally known to differ by very small quantities, if at all, from these common values.

This is the plain, unquestionable result of all the chemical work of the nineteenth century in atomic weight determinations.

Hence the real problem to be solved is the determination of the exact, but small, departure of the true atomic weight from these common values.

Standard Atomic Weights.

These common atomic weights of whole numbers (or in a few cases exact half units) shall in our calculations be taken as the *absolutely fixed standards of comparison.*

We therefore shall call them *the standard atomic weights.*

The following table gives these values, carefully revised by myself, and in alphabetical order of the chemical symbol.

Table of Standard Atomic Weights.

A		C	12	Fe	56	Ka	39	Os	191	Se	79	Ur	240
Ag	108	Ca	40	Fl	19	La		P	31	Si	28	Va	51
Al	27	Cd	112	Ga		Li	7	Pb	207	Sn	118	Wo	184
As	75	Ce		Ge		Mg	24	Pd		Sr		Zn	65
Au	197	Cl	35.5	H	1	Mn	55	Pt	195	Ta		Zr	90
Ba	137	Co		He		Mo	96	Rb		Te			
Be	9	Cr	52	Hg	200	N	14	Rh		Th			
Bi	208	Cs		In	113.5	Na	23	Ru		Ti	48		
Bo	11	Cu	63.5	Io	127	Ni	58	S	32	Tl	204		
Br	80	Di		Ir	193	O	16	Sb	120	Tu	(see Wo.)		

The mathematical problem to be solved by means of a thorough discussion of *all the actual analyses* made by the chemists of the nineteenth century, is the determination of the *departure* of the *true* atomic weights from these fixed standard values.

Surely, if we establish, *from the actual analyses*, the exact value of such departure, we shall have determined the true atomic weight for any given element.

To most chemists this may seem to be an indirect method, a round about method; but a moments consideration will convince them that this method is not only direct, but the only mathematical method applicable to this problem.

Instead of complicating the calculation, as might be supposed, this method *simplifies all calculations* to a wonderful degree.

In fact, it may be truly said, that the direct solution of this problem of determining the true atomic weights is *impossible*. In all fairness, the chemists who for a century have tried their best by this method and now see the whole subject in a muddle and no single atomic weight truly known, ought to be ready to concede that *their direct method has been a failure*.

Now, wherein is our indirect method, if the chemists will call it such, simpler than the direct method thus far employed by the chemists of the past century?

It is due to the *fact* that the deviations from the standard values being known to be *small quantities*, the method of calculation becomes extremely simple, *because second and higher powers of these deviations can be neglected*.

Method of Procedure.

Let us now see how this our method can be applied in the most simple manner for the determination of the absolute and true atomic weight of the chemical elements.

By the analytical operation of the chemist, the element is weighed in two different combinations, the one having been changed into the other without loss or gain as near as possible. The weighings are exact, as near as can be.

By the series of analytical determinations, that is, by the *laboratory work*, we obtain as many *analytical ratios* as determinations have been made; namely, in each single case we divide the weight of the *substance* taken, s, by the weight or the *product* formed, p; the quotient is our *ana-*

lytical ratio, a, and is calculated to *five* decimal places, uniformly, in this book.

$$s : p = a \qquad (1)$$

Now both the substance and the product are definite, well known chemical compounds, as pure as it is possible for the most refined chemical art to produce them.

Hence the chemical formula of these compounds is known.

Taking our *standard atomic weights* for these symbols, we shall obtain the *standard atomic weight* of both the substance and the product.

Let us represent these known *numbers* by S and P, then a simple division will give us the *standard atomic ratio,* which we also calculate to five places:

$$S : P = r \qquad (2)$$

All the rest is done by simply comparing the analytical ratios to the atomic ratio, always using the units in the *fifth place* for this purpose.

We shall soon learn that, *as a matter of fact,* the analytical excess

$$e = a - r \qquad (3)$$

which is the difference between the analytical and our atomic ratio, is very small, in all cases where the analytical work has been done by a really good practical chemist, and where the method used has been a good, well tested, method.

In order to avoid the use of the signs minus and plus, always awkward in non-mathematical books, we shall use the common terms *high* and *low* to designate the character of the analytical excess e.

Namely *high* if it is greater, and *low* if it is less, than the atomic ratio.

Example: Mercury.

For example, mercuric oxide HgO yields metallic mercury, Hg; both are accurately determined under the conditions worked out by Erdmann and Marchand in 1844.

The standard atomic weights are $Hg = 200$, $O = 16$, exactly. Hence the reaction determines the atomic ratio r as follows: $Hg : HgO = 200 : 216 = 0.92\,593$.

Here the *atomic ratio* is r = 0.92 593.

To secure ready comparisons, we shall always print these ratios by leaving an n-space between the second and the third decimal.

In this manner the five-place decimal becomes easily readable, the *first two decimals* represent the per cent., the *last three decimals* represent the tenth, hundredth and thousandth of per cent.

We know of no chemical work where the sixth decimal is actually determined, or significant. Hence we never shall give more than five decimals.

In the *first* determination by Erdmann and Marchand, they obtained 75.9347 grammes of metallic mercury from 82.0079 grammes of mercuric oxide; accordingly, we obtain the *analytical ratio*, by dividing the first by the second weight, a = 0.92 594.

Evidently, in this their first determination, the analytical excess is *1 high* according to our mode of expression; for the last decimal of the analytical ratio is 4, while in the atomic ratio it is 3.

In this manner, every statement of fact is reduced to the simplest possible form, and easily grasped by the mind.

Extremes and Range.

We shall also have to specially consider the *extremes* and the *range* of the analytical ratios of any series. We shall invariably give the *highest first*, then a *dash* as minus sign, followed by the *lowest* value observed. A *semicolon* followed by the *range* completes the statement. Having to give a multitude of results, brevity and uniformity of representation become very important.

Since in good series of determinations there are no changes in the first two decimals, it would be absurd to incumber the record therewith; hence we only print the *last three decimals* of the extremes.

In the case of mercuric oxide, Erdmann and Marchand found the *highest* analytical ratio 0.92 606 and the *lowest* 0.92 594.

Hence we record simply thus: Extr. 606—594; 12.

We may even omit the *Extr.* without causing any confusion.

Determination by Sight.

In this manner it becomes a *simple matter of inspection* to ascertain *how closely* the actual experimental determinations, expressed in the analytical ratios, approach to the atomic ratio, calculated from the standard atomic weights.

If the observed ratios differ more among themselves than from the atomic ratio, then the atomic ratio expresses the facts observed within the limit of actual determinations.

In this way we shall find whether or not the standard atomic weights are the true atomic weights.

If the analytical ratios, the only direct expression of the observed facts, agree within the limit of accuracy obtained with the atomic ratio, then the true and the standard atomic weights are necessarily the same within the limit of accuracy obtained by the actual experimental determinations made.

It is in this simple matter of fact manner that we are now able to test all the atomic weight determinations made during the entire nineteenth century.

Order of Procedure.

We shall first consider the most important of all elements, and mainly the work of the old master, Berzelius, and his school, in which no fancy method of work was tolerated, and when simple appliances in skillful hands, directed by clear heads gave results that still challenge admiration.

Having become versed in this work and acquired confidence in this method, we shall next apply it to the determinations of the atomic weight of boron made in the best Laboratories of London and Paris, by or under the immediate direction of the most famous operating chemists of the present, namely, by Ramsay and by Moissan. The work of the latter has been endorsed by the Academy of Sciences of Paris.

We shall then be able to settle the question of the true atomic weight of nitrogen, the corner stone of the system of Stas and his school.

Then will follow the complete record of all experimental determinations made during the century, in alphabetic order of the symbols of the elements.

Atomic Weight Calculation Made Easy.

We have not yet shown how the exact atomic weight corresponding to any given analytical ratio can instantly be obtained by a simple mental calculation. This is due to the fact that we here really are making use of a very refined method of mathematical analysis, although we wish the chemical reader not to get aware of it—for he might shy.

We may suppose that every body understands that all quantitative relations can be graphically represented by a curve drawn to scale, and that at any point of such a curve the element of the curve may be considered a *straight line* (the tangent) for a distance sufficiently short.

But then the changes of the variables, the co-ordinates, will be directly proportional within that limit.

Hence, for small changes *the analytical excess will be directly proportional to the corresponding change in the atomic weight.*

Now nothing is easier than to determine and express this change in a uniform manner. For we need only calculate the atomic ratio say *for an increase of 0.1* of the standard atomic weight, to find the change in atomic weight corresponding to any analytical excess.

In the above instance, we found the *standard* atomic ratio.

$$\text{Hg} : \text{Hg O} = 200 : 216 = 0.92\ 593.$$

Suppose now that the *true* atomic weight of mercury were 200.1, then the true atomic ratio would be

$$\text{Hg} : \text{Hg O} = 200.1 : 216.1 = 0.92\ 596.$$

The supposed *true* atomic ratio in this case would simply be "*3 high*" as compared to the *standard* atomic ratio, using our simple method of expression for the excess being 3 units in the fifth place.

We may also express this result by saying

"Hg = 200.1 gives ratio 3 high" or "change of 0.1 gives ratio 3 high" or "*Chg. 3 high.*"

Now, in the first determination by Erdmann and Marchand, they found, as above stated, the analytical ratio 0.92 594 or "1 high."

Since 0.1 causes 3 high, this actual "1 high" corresponds to our one third of 0.1 or 0.03 on the atomic weight of mercury.

That is, by a mere glance at the analytical excess (here 1 high) the calculated *change* (always for 0.1) gives the corresponding *departure* of the atomic weight from the standard.

In this case, for this first determination by Erdmann and Marchand, *departure* is 0.03 from the *standard* 200, so that the atomic weight of mercury *exactly corresponding* to that first determination is 200.03.

It is plain, that this method is the simplest possible for use, calling for no calculation but such as can be instantly made mentally, the *changes for 0.1* having been given.

It is the well known method of proportional parts, used in all common tables of sines, tangents, logarithms—we extend it to the atomic weight calculations.

Of course, the *possibility* of doing this depends upon the *fact* that *the true atomic weights differ very little from our standard atomic weights*, as we have recognized it in all the analyses of the nineteenth century so far as the chemists were able, and therefore their methods used, reliable.

Now, if absolutely reliable and practically concordant analyses should give any appreciable analytical excess, not due to errors of work or process, then we can instantly, by the above proportional parts, mentally calculate the exact *departure d* of the *true atomic weight* t from our *standard atomic weight* s and obtain $t = s + d$ (4)

Standard and True Atomic Weights.

I may already here remark, that we shall find this analytical excess e entirely within the limit of precision attained, zero, and consequently also the departure d will be zero, and therefore *the true atomic weights are identical with our standard atomic weights.*

This is the grand final result of this, our analysis of all atomic weight determinations made up to the present date.

Our Earlier Publications.

In conclusion, we may be permitted to point out the steps which have led us up to this, the simplest and most direct method, which I trust will be within the easy comprehension and application of every student of chemistry in the world·

We shall simply indicate our leading publications concerned.

Our work, "The True Atomic Weights of the Chemical Elements and the Unity of Matter, St. Louis, 1894," gives essentially this method, but not by itself, since it was my aim also to show *how* eminent analysts had been mislead; hence I entered upon the consideration of "the trajectory of errors" and the mathematical principles of "the limit method."

Many chemists, unable or unwilling to understand these collateral matters, have shown by their manner that I was altogether too tender in this fight for truth against error and fraud.

Hence I have, in this present work, exclusively devoted myself to show in a manner so plain that the wayfaring man even though somewhat foolish need not err.

Indeed I trust every chemist will see that the *result*s embodied in the Stasian methods and atomic weights *are false*, and that *as a matter of plain fact, our standard atomic weights are the true atomic weights within the degree of precision actually attained.*

The entire method, in its essential feature, has been printed in my "General Method for the calculation of Atomic Weights from the Results of Chemical Analysis" in the Comptes Rendus, T. 116, pp. 695–698; 1893, which publication was followed by several applications of the method, in other issues of the Comptes Rendus.

The general mathematical principles upon which all this depends may be found in my "Method of Quantitative Induction," Davenport, 1872, and much of the detail of calculations involved in the various branches of this work can be found in my "School Laboratory of Physical Science, vol. I, pp. 88–93; and pp. 93–99; 1871."

It was that very work, elementary but on a very large scale, carried on by hundreds of students under my care, that made me understand the fallacies of probable error, means, etc., which fallacies constitute the dark cloud that has been resting over this part of chemistry since Stas began his work, and infected modern chemists with the horrible Morbus Stasii.

Baculus vs. Bacillus.

In my "True Atomic Weights" I tried to show how these victims of Morbus Stasii might have contracted this horrible disease by mistaking the Bacillus thereof for some beneficent agent on their "Means" and thus encouraging them to commit heinous "Probable Errors" far beyond their "Limits," thereby getting away from the path of truth on some jag-like "trajectory" of errors and deviations.

As this my kindness of heart has been either mistaken or wilfully misconstrued by the victims corrupted in their scientific vitals by Morbus Stasii, I have become convinced of the duty to use a plain, strong Baculus energetically, so as to drive out and kill the Bacilli from the old victims, and thus to prevent the young chemists of the world from infection by the horrible Bacilli of Morbus Stasii.

THE WEIGHT OF A HALF EAGLE.

Before actually beginning our work of absolute atomic weight determinations, it may be advisable to supplement our experimental determination of the weight of a silver dollar by a corresponding experimental determination of some United States Gold Coin.

In this way we shall see, by contrast, the effect of the greater value of the material used in producing much more accurate work at the mint and a consequent greater accuracy in our determination.

This corresponds exactly to the difference between ordinary chemical analysis and atomic weight determinations.

Roughly speaking, the work on gold coins is ten times as accurate as the corresponding work on the silver coins. Accordingly we have to weigh to the milligramme.

We shall restrict ourselves to such subjects of this investigation as are immediately applicable to our atomic weight determinations.

The Mean Weight of the Half Eagle.

The most suitable United States Gold Coin for this study is the *Half Eagle*, corresponding to the English *Sovereign* and to the German *Twenty Mark* coin. Its value is five dollars. It is the most common gold coin of the world.

Drawing six such coins at a time at the Bank, I have gradually obtained over one hundred such coins, exactly as they were current during the first six months of this year, 1901.

Each coin was weighed to the milligramme and the *mean* of each lot of six coins was calculated. The following table gives these *means in lots of sixes* in the order of time:

8.298 — 8.328 — 8.346 — 8.359 — 8.331 — 8.337 — 8.340 — 8.348 — 8.340 — 8.343 — 8.337 — 8.340 — 8.333 — 8.349 — 8.339 — 8.327 — 8.334 —

These means vary quite considerably. The first is the lowest, 8.298; the fourth is the highest, 8.359. The range of these means (of six each) is 0.061 or 61 milligrammes.

The *mean weight*, of six each, *is not the true weight*, very evidently.

Let us see, how the means will run if we combine consecutively two of the groups, so as to get the means of 12, then of 24, and lastly of 48 coins, in the order of time, exactly as they came gradually to hand.

The *means of twelve coins* each are: 8.313 — 8.352 — 8.334 — 8.344 — 8.341 — 8.339 — 8.341 — 8.333.

These means of twelve agree, of course, much better. The first is the lowest, 8.313; the second is the highest, 8.352. The range is 0.039 or 39 milligrammes; only about one half of the range of the means of sixes.

Combining again these means two and two consecutively, we obtain the *mean weight* by twenty fours of Half Eagles:

8.333 — 8.339 — 8.340 — 8.337.

These agree much closer again, the entire range being only 7 milligrammes.

The means, by forty eight Half Eagles are: 8.336 and 8.338, differing by only 2 milligrammes.

The mean of these two is 8.337 for 96 Half Eagles weighed.

We see how gradually the mean becomes more fixed, less subject to fluctuation, as the number of individuals used for that mean increases.

This has led scientists to suppose, that we obtain a higher accuracy as we increase the number of observations.

We gain concordance—expressible by a small probable error—but we have not approached the true weight. We shall find this very mean 22 milligrammes low as a very sound constant error.

We understand the fallacy of this common conclusion. The same number of Half Eagles in another city would *not* even have given the same results, since other years of coinage most likely are more frequent in other towns, and above all, the actual weight of all Half Eagles in circulation is *low*, due to abrasion. This will vary greatly in time and rapidity of circulation—and for gold coin also with the greater or less care exercised in withdrawing light coins from circulation. The United States are not very particular in this matter.

Out of the 102 Half Eagles weighed, the following were light coins:

8.235 — 8.235 — 8.257 — 8.270 — 8.280 — 8.282 — 8.284 — 8.296 or about 8 per cent below 8.300.

As a matter of fact, the actual coin in common use has a larger number of light weights than this percentage, because the teller did not hand me any coin but such as he considered good coin.

Frequency of Circulation.

The oldest Half Eagle in this lot was of 1857, the latest of 1901. The entire period comprises 45 years.

How remarkably varied the frequency of the coin is, we found again for gold as we have found it for silver.

The year 1880 was represented by 9 coins, 1895 by 8; these two years by 17 coins out of 102.

The year 1881 was represented by 20 coins, the year 1897 by 21 coins; these two years represented 41 coins out of a total of 102.

The three years: 1880, 1881 and 1897 were represented by 50 coins. In other words, these three years had furnished half of all coins in local circulation; as many as the other 42 years taken together!

Amount of Abrasion.

The great frequence of the coins of the two years 1881 and 1897 permits us to obtain an estimate of the amount of abrasion.

But upon looking over the record of the individual weights of the Half Eagles of 1897 we find one decidedly under-weight, namely 8.235 only. This exceptionally "worn" coin must, therefore, be laid aside. The twenty remaining coins of 1897 range from 8.322 to 8.370, and give a mean weight of 8.357.

The twenty Half Eagles of 1881 run from 8.300 to 8.356 and give a mean of 8.333.

Hence, in the 16 years from 1881 to 1897, the mean wear of 20 Half Eagles has amounted to 24 milligrammes, which is 1½ milligramme per year.

Therefore, a new Half Eagle, in 1901 should weigh about 4½ mgr. more than the mean for 1897; that is 8.361 grammes; for those of 1897 have only been 3 years in circulation.

We have obtained only one single Half Eagle of 1901; it weighed 8.356 grammes, but of course, had lost some by abrasion of perhaps half a year. Besides, the mint cannot produce the coin equal, even of gold, to the milligramme.

Not having had the opportunity of weighing, say at least 20 such coins, fresh from the mint, I cannot state the *actual* tolerance. I think it must amount to 5 mgrs.

Now, by Law, each Half Eagle is to weigh 129 grains, that is (to nearest milligramme) 8.359 grammes.

Our estimate, based upon 20 coins each of 1881 and 1897, gave us 8.361 or 2 mgr. in excess of the mean legal weight. I think we have done well enough. It corresponds to 0.02 on an atomic weight of 83.

But really, we did *not* determine the weight directly. Direct means were all *low*; for 1881 they were 24 mgrs. below the mean for 1897.

These two years gave us the average *rate of wear* or abrasion. We had no new coin, fresh from the mint. We *supposed* that the rate from 1881 to 1897 might be relied on as reasonably true—and hence as such beyond our actual observation, from 1897 to the present. That gave us the weight 8.361 at the mint; the law says it shall be 8.359.

We think there is no flaw in this process—beyond the desirability of larger numbers of coin. That desirability we admit. In fact, we admit it very much.

Criminal Extrapolation.

I am sorry to inform my readers, that they have been participants in a great scientific crime, the crime of *Extrapolation*.

Possibly they have not felt their scientific conscience shiver; that would be too bad, according to the opinion of the great Stasian critics, referred to by the Olla Podrida maker for the Smithsonian Institution, on page 6 of his variable Constants of 1897.

This scientific crime of extrapolation consists in carrying experimental data beyond the immediate field for which they have been established.

Thus Stas claims he found 14.04 for nitrogen, using from about 100 to 400 grammes of silver, converting it to nitrate, fused and dried. As a matter of fact (True Atomic Weights,

p. 164) the corresponding atomic weights of nitrogen varied all the way from 14.05 to 14.10.

Yet the Stasians, referred to by Clarke above, and all other Stasians have for forty years extrapolated the atomic weight of nitrogen for *any* weight of silver, below 100 grammes and above 400 grammes.

In fact, the Stasians, are hardened criminals in this matter of extrapolation as well as in all other scientific crimes.

But what is the use referring to such things any longer? Why suppose it possible that such men, "blind followers of a blind guide," will want to see light? Matt. XXIII.

Hence, let us say just a few words on this great crime of extrapolation we have committed above—and exactly in the same way in our limit method of our True Atomic Weights of 1894—and at almost every step we have taken in our life!

Truly, every step is an extrapolation; we do not know that the laws under which nature worked yesterday, will remain to-day. We do not know that the sun will rise to-morrow; if we say it will, we commit the horrible crime of extrapolation!

Come to think of it, I was instructed in this scientific sin of extrapolation by my own father; when a mere boy, helping him surveying I had to prolong a line by setting stakes in continuation of two stakes—and surely, that is extrapolation of the worst kind! But I remember I did it well; probably natural depravity aided by parental authority and instruction.

In fact, Euclid and other old Greek heretics, inculcate the same sinful operations.

It is really grotesque to hear "the blind followers of the blind guide" speak of the crime of extrapolation from out of the mire of error and fraud in which they have complacently weltered like a lot of the most common pachyderms of these prairies. How deliciously dainty the Greek sounds in this place.

The whole Stasian system being a mysterious muddle of error and fraud, varnished with a pharisaical show of sham-exactness, is true to itself in crying: *you extrapolate!*

When Newton is said to have thought "*if* that falling apple had come from the moon" was not he guilty of extrapolation? But his extrapolation we call the law of universal gravitation, and it has been so called for two centuries.

If you have the truth, you may extrapolate, and nature will vindicate you.

But if you are surrounded by frauds and lies, and if your very soul has been filled by this contamination, then do not get out of the miry hole, *do not extrapolate* either yourself or anything else next to you, for the Light of God's Sun will show to all the world where you have been and what you are!

The Ratio and the Excess.

And now finally—let us apply this little lesson also in a strictly formal way, in numbers and by calculation, exactly as if we had an "absolute" atomic weight ratio to compare with the "analytical ratios" of actual experience.

Here we have the legal weight of the Half Eagle as the absolute standard of comparison, namely 8.359 grammes. That is our unit.

We have also the present actual "mean weight" of 20 Half Eagles of 1881, being 8.333 grammes.

We have likewise the actual "mean weight" of 20 Half Eagles of 1897, namely 8.357 grammes.

We had to reject one of the coins of 1897 weighing only 8.235.

We finally obtained—by the dreadfully criminal operation of extrapolation—the estimated weight of a new Half Eagle at the U. S. Mint to be 8.361, although we regretfully admit we ne'r had such an one in hand.

Let us calculate the corresponding "analytical ratios" to our usual five places, exactly as we do in our absolute atomic weight determinations. That will give us a very useful case of comparison, from the best work that can be done at the mint, on the most valuable material, gold, when again picked up after having circulated in this sinful world at large, and brought upon the balance and tested "only to the milligramme," not to the thousandth of the milligramme!

Actual determinations of Half Eagles the legal weight being taken as standard.

	Ratio.	Excess.
20 Half Eagles, of 1881, mean,	0.99 689	311 low.
20 Half Eagles, of 1897, mean,	0.99 976	24 low.
Estimated Weight at mint	1.00 024	24 high.
Legal Weight shall be	1.00 000	0 high.
Rejected coin of 1897,	0.99 593	407 low.
Amount of Abrasion, 16 years,		287
hence per year,		18

These ratios and the corresponding excesses are very instructive. They show *how very rigid the comparison of ratios to the fifth place is.* Our carefully made estimate of the new coin at the mint, differing only by 2 milligrammes from the legal standard, here shows up with an excess of 24!

We see also that the "rejected" coin of 1897 fell almost a hundred below the analytical excess of the mean for 1881.

Again, it appears strikingly, that the mean weight of the gold coin gradually approaches the legal weight, as the year of coinage is less and less distant from the present. The mean of 1881 was 311 low, that of 1897 only 24 low.

It will be well to keep these cases before our eyes throughout the study of this work.

II. THE ATOMIC WEIGHT OF LEAD. BERZELIUS.

The atomic weight of lead we find to be fully established by the splendid determinations made by Berzelius more than seventy years ago. The many later determinations have only clouded the work of Berzelius for a time. We therefore put his name at the head of this section, hoping that hereafter due credit will be given this our great master for the experimental determinations which have definitely and permanently established the true atomic weight of lead, the metal of Saturn.

A. Lead Carbonate Ignited.

The earliest work of Berzelius I can find recorded on the ignition of the carbonate, yielded 83.5 per cent of lead oxide. Philos. Transact. 1814; I quote from Becker (*) p. 71. In this work Berzelius was assisted by F. H. Wollaston.

Per unit of weight of lead carbonate, the above analysis gives 0.835 as the analytical ratio.

By our standard atomic weights, $PbO_3C = 267$ and $PbO = 223$, hence our atomic ratio is

$$223 : 267 = 0.83\ 521$$

and repeating the calculation for $Pb = 207.1$ we find

$$223.1 : 267.1 = 0.83\ 527$$

which is 6 higher. Hence, in our manner of expression, "Change 6 high" for a rise of 0.1 in the atomic weight of lead.

With these standard ratios the actual analytical work has to be compared.

It will be seen, that every digit of the analytical ratio of Berzelius is exact; it is 0.835. And this dates back to 1814!

We find a second double determination in Meyer and Seubert, p. 128; the individual values are never quoted by M. & S., they only give aggregates. We can, however, trace the exact value of the analytical ratio.

The two determinations were made by Berzelius in 1817. He took 10 grammes of carbonate in each determination; the sum of lead oxide stated is 16.6666 grammes.

It is also stated, that the carbonate was found to contain a trace of moisture; 0.0225 in the first, 0.022 in the second, hence 0.0445 in the two. Accordingly, the actual amount of real lead carbonate was not 20 grammes exactly, but only 19.9555.

Dividing the amount of lead oxide, 16.6666 by the amount of lead carbonate 19.9555 we obtain 0.83 517 as the analytical ratio of Berzelius two analyses of 1817.

It was with astonishment that I beheld this number; I revised my calculation, being unprepared for such wonderful result.

---

* The so-called *Recalculators*, mainly of the work of Stas, but also of all existing atomic weight determinations, are:

Becker, Meyer and Seubert, Sebelien, Clarke, Ostwald, Van der Plaats, and Julius Thomsen. The full title of their works will be given at some one place. See True Atomic Weights, 1894, pp. 40–69.

I have just revised the calculation once more, before going to press; the result is exactly as stated.

Simple inspection shows:

Atomic Ratio, . 0.83 521 Hinrichs, 1901.
Analytical Ratio, . 0.83 517 Berzelius, 1817.
or, in our parlance, . . "*4 low*" only in the fifth place!

Now since our "change is 6 high" corresponding to 0.1, the atomic weight corresponding to the two determinations of Berzelius, made in 1817, is $\frac{4}{6}$ or $\frac{2}{3}$ of 0.1 low, that is 0.06 low. Accordingly Pb = 206.94.

That is, the experimental determinations of Berzelius, *taken to be absolutely exact*, would correspond to the atomic weight of lead being 0.06 *less* than the standard 207; that is Pb = 206.94.

But Berzelius himself would never assume absolute accuracy for his work. We see then, that his oldest determinations of the carbonate on record *agree within the errors of experiment with the standard atomic weight of lead, 207.*

And these errors of experiment we have found to be not in excess of 0.06.

Contrast herewith "the most recent work" tabulated by Clarke in his edition of 1897, for all determinations of lead, ranging 2.5 units, instead of 0.06, or forty times the uncertainty of the work of Berzelius done in 1817!

B. Lead Oxide, Wet Way.

In the earliest determinations on the conversion of lead into lead oxide, Berzelius generally started with ten grammes of lead, dissolved the same in a glass matrass with long neck by nitric acid, and converted the resulting nitrate by ignition into the oxide in the same matrass. Special variations in the general process we need not refer to here.

The formula of this process is

$$\text{Pb O} : \text{Pb} = 223 : 207 = 1.07\ 730.$$

A rise of 0.1 in the atomic weight of lead causes a rise of 6 units in the fifth place of this ratio; that is, gives 6 high.

Berzelius found ten grammes of lead to gain 77.5 centigrammes when evaporated in the same glass vessel, and 78 cgr. when using a matrass with very long neck. These results were first published by him in 1810. (Sebelien, p. 143).

Per unit we get the analytical ratio 1.07 750 (where we have to supply the last decimal not determined) which is 20 high.

The actual determinations to the fourth place, which really is uncertain, since Berzelius weighed to the centigramme only which represents the third place in our ratio (centigramme to 10 grammes is milligramme to the gramme, or third place in our ratio per unit).

In other words, Berzelius found the oxide exceeding the amount calculated from the standard atomic weight only two centigrammes in excess on ten grammes of lead; that is, only two units of his smallest weights used on his balance.

These are in point of time, the earliest of all atomic weight determinations of any heavy metal; they differ from the calculated standard by only two units in a thousand.

C. Lead Oxide, Dry Way. Earliest Work.

In 1812 he published in the Swedish Afhandlingar (vol. V, see Sebelien p. 143) his first reductions of pure lead oxide to metallic lead by heating the oxide in a current of dry hydrogen gas.

This is a chemical process of a much higher degree of accuracy, and accordingly we find that his determinations came much nearer our atomic ratio.

His three determinations, referred to a unit weight of lead, gave 1.07 722; 1.07 723 and 1.07 740 as analytical ratios.

These ratios are, in the order given, 8 low, 7 low and 10 high, when compared to our atomic ratio above given, namely 1.07 730.

The mean of his three determinations is 1.07 728 which is only 2 low (in 5th place) in comparison with our atomic ratio.

If we were to consider his determinations exact to the fifth place of the analytical ratio, the atomic weight would be $\frac{2}{6}$ or $\frac{1}{3}$ of one tenth, that is 0.03 lower than the standard 207, that is 206.97.

Hence we can assert with absolute certainty that the three determinations of Berzelius made by reducing lead oxide by hydrogen, conform to the full limit of the precision of his work to the standard atomic weight of lead, Pb = 207.

The deviation of the mean is only 2 low (in the fifth place) and the individual determinations fall on either side, two being 8 and 7 low, one being 10 high (in the fifth place).

These dry way determinations of Berzelius, published in 1810 and 1812, in the very beginning of any atomic weight determinations, fix the value of the atomic weight of lead at 207, within a very few hundredths as a barely possible uncertainty.

This is the record of our Science. Here we have the earliest record of the work done by the greatest master in chemistry. Every true chemist should be proud of this record.

Clarke Falsifying the Record of Berzelius.

It is therefore with inexpressible disgust and contempt that I read, at the very opening of the report on the atomic weight of lead, in the Smithsonian publication of the Chief Chemist Clarke (Constants of Nature, Washington, 1897) at the top of page 127, the following which, every reader of the historic facts just given will recognize as barefaced and absolute falsehoods:

"The researches of Berzelius upon the carbonate and "various organic salts *need not now* be considered, *nor is it* "*worth while to take into account any work of his done before* "*the year 1818.*"

The work of Berzelius on Lead Carbonate came within 3 units in the fifth place. This dry way work on the oxide came within 2 units of the fifth place of our atomic ratio.

In both cases this means the determination of the atomic weight of lead to be 207 within a possible range of only three hundredths of a unit.

At the close of his chapter on lead, this same *Chief Chemist Clarke* states the result of all subsequent work on lead to run, in the *mean values over two and one half units.*

This is a range much in excess of *eighty times* that affecting the determinations of Berzelius made from 1810 to 1814 on the carbonate and the oxide of lead.

I trust that every young chemist will read the protest against Clarke's garbling the record of Berzelius I have published in my little book on "the False Atomic Weights of the Smithsonian Institution," pages 28 to 30.

They will, I am sure, agree with me in that protest against the wilful and malicious defilement of the grand work of Berzelius.

It is nothing short of a disgrace that the highest scientific officers of our government dare produce such totally false statements of the record of Berzelius, and that such disgraceful falsehoods are published with the endorsement of the Secretary by the "Institution" founded for the Increase and Diffusion of "*Knowledge*" among men, and thus sent broadcast to chemists everywhere at the expense of the people of the United States.

D. Lead Oxide, Dry Way. Later Work.

The work of Berzelius which we shall now consider is referred to his Lehrbuch, last or fifth edition, volume III, p. 1218, by Clarke, and divided into two parts marked "earlier" and "latest" results. The former comprise 6 determinations, the latter 3 determinations.

Meyer and Seubert (p. 28) refer to the same "Lehrbuch" and give the years of first publication as 1830 and 1845.

Becker (p. 71) mentions "four nearly coincident experiments" published in Poggendorff's Annalen for 1826, meaning Pb = 207.12 for O = 16. He also refers to "six experiments" under the heading 207.078, as published in the same Annalen in 1830. As last reference he says that Berzelius "selected" five of the preceding analyses giving 207.14 and gives as source for this reference the same volume and page of the Lehrbuch above mentioned.

Turning to the Skandinavian Sebelien (p. 145–146) we find the identical nine determinations quoted by Clarke—identified by the weights given—but in a different order, and here referred to the Transactions of the Swedish Academy for 1830.

In the absence of the original publications (we have only the 3rd edition of the Lehrbuch and a French translation published in Brussels, and neither the Annalen nor the Handlingar) we conclude that all these determinations were made at or before 1830.

Both the German and the American authors (re-calculators) are apparently in error in making the last (fifth) edition of the Lehrbuch a date mark for this great work of the Skandinavian Chemist Berzelius.

Evidently, Berzelius in the last edition of his great work, *merely summarized the result*, by using only five of his nine determinations.

Sebelien mentions this exclusion of four determinations of the nine, as having been made by Berzelius because they differed most from the mean.

Sebelien specifies the very ones excluded, which we find all to belong to the list marked "earlier" by Clarke, and forming the first four of that list.

We therefore feel authorized to subdivide that "earlier" list as shown below.

The four determinations excluded by Berzelius from his Lehrbuch are apparently the oldest determinations, referred to by Becker as published in the Annalen in 1826.

The other five determinations (not 6 as Becker has it) were published in 1830, both in the Handlingar at Stockholm, and in the Annalen at Berlin.

None of this work is later than 1830.

The impression made by the German and American Recalculators, that some of this work of Berzelius was as late as 1845 at least, is surely an absolute error.

Considering the great importance of this series of determinations made by Berzelius, we deemed it necessary (and just to Berzelius) to establish the date of this work.

We are convinced that this work will be looked upon as the finest and most important quantitative work of the Century.

We now give the weight (in grammes) of the nine determinations made by Berzelius, and published respectively in 1826 and 1830.

The weights are not reduced to vacuum, though Berzelius satisfied himself of the influence of as large a charge as 20 grammes of the oxide on the loss in weight.

The high specific gravity of both the lead and its oxide, made the omission of such reduction insignificant.

Berzelius' Reductions of Lead Oxide.

Year.	Oxide.	Metal.	Analytical Ratio.
1826	8.045	7.4675	0.92 822
"	14.183	13.165	0.92 822
"	10.8645	10.084	0.92 816
"	13.1465	12.2045	0.92 835
1830	21.9425	20.3695	0.92 831
"	11.159	10.359	0.92 831
"	6.6155	6.141	0.92 828
"	14.487	13.448	0.92 828
"	14.626	13.5775	0.92 831

This column of *analytical ratios*, the real expression of the experimental work done, is certainly marvelous.

The mean of the four oldest determinations is 0.92 824, with a range of 19 in the 5th place, in 1826.

The mean of the five later determinations is 0.92 830, with a range of 3 only—in 1830.

The mean of all determinations made over seventy years ago, is 0.92 827.

Now let us see what the atomic ratio for this process is.

Pb : Pb O = 207 : 223 = 0.92 825. Change 3 high.

This atomic ratio is practically identical with the analytical ratios found by Berzelius in and before 1830.

The mean of the determinations of 1826 is *one low* in the 5th place.

The mean of the determinations of 1830 is *five high* in the 5th place.

The mean of all determinations is *two high* in the 5th place.

The individual deviations of the analytical from the atomic ratio are, in the order stated:

1826: 3 low; 3 low; 9 low; 10 high.
1830: 6 high; 3 high; 3 high; 6 high; 2 high.

The earlier determinations fall almost equally on both sides of the atomic ratio.

The later determinations are all high: 2 high, once; 3 high, twice; 6 high, twice.

Take the entire series, and the individual values of the analytical ratios are identical in the first three decimals, while the last two are, in the order of magnitude

16, once; 22, twice; 28, twice; 31, thrice; 35, once.

They are properly distributed about the mean value (27) to allow the calculation of the probable error of the mean.

This probable error is 1.4 in the 5th place.

Surely, the atomic ratio is established as the true ratio by these analytical ratios.

These determinations of Berzelius leave no possible room for the supposition that the deviation of the atomic weight of lead from the standard 207 is anything but zero.

Hence, these determinations of Berzelius demonstrate that the true atomic weight of lead is 207 exactly.

But why has this fact not been recognized, since these experimental determinations of Berzelius have been known for three quarters of a century?

Very simply, because chemists, even Berzelius himself not excepted, took each individual determination and from it calculated the atomic weight of lead—far beyond the degree of precision warranted.

It is well known that even Berzelius himself carried these calculations, for his large unit of oxygen = 100, to two or three decimals.

For lead 207 this gives 1293.75 in Berzelius' units.

His own calculations, as reported by Sebelien (p. 146) from these determinations ran from 1292.000 to 1294.946, a range of 2.946.

Now Berzelius must have had frequent occasions to notice that what seems perfect and finely finished to the naked eye, need only be looked at through a magnifier—and it will appear to be rough and coarse.

The edge of a razor is very fine—but under a microscope it looks like a worn out saw.

Berzelius unwittingly magnified his errors—and then felt like counting out some of them.

The great chemist was a good calculator, and liked to obtain six or seven digits in his final result. Possibly he used "seven place logarithms." See pp. 44–45.

The great chemist was not of a mathematical turn of mind—not any more than his very noted re-calculators.

But while he was unable to stop at the right time at the proper place in the string of decimals, he knew all about the chemical work.

While he was as reckless in carrying the calculation of additional decimals beyond the limit of his own chemical determination as any of the modern chemists, the re-calculators included, his fine chemical sense, if we so may call it, did not permit him to introduce wilfully corrections less in amount than the uncertainty of his real chemical work.

It was, I believe, in reference to this investigation that Berzelius made the statement about the chemists who strained at a gnat while swallowing camels. (Sebelien, p. 45; Matthew XXIII, 24).

Sebelien (p. 45) positively asserts, that Berzelius never reduced his weighings to vacuum, because "he had found "that the single determinations deviated to a much greater "extent than the amount of such correction."

This is just so to-day; but we like to *pretend* to be exact, we create the *show* of a high degree of accuracy—and do not, all of us, realize how far we modern chemists with balances permitting us to "oscillate" to the hundredth or less of a milligramme are away off in the woods.

I trust that this little book will make chemists again go to the Grand Old Swede to learn how to work to the advantage of truth.

E. How I Learnt the Name Berzelius.

Early in the summer of 1853 I came to Copenhagen. I had never been away from home; my health would not have permitted, if means had allowed.

I had just passed my examinations for admission in the halls of the university, together with many other young men, who had enjoyed the advantages of higher schools for many years. I had for one year been the happy possessor of a couple of Danish works on Elementary Mathematics and a Danish grammar and a dictionary—to learn both the language and the science—without a master.

After Professor Ramus had examined me in mathematics, one of the older hands whispered to me: "you did very well." I asked, "how can you tell?" when he called my attention to the significant fact, that the Professor had kept one of his boots on, entirely undisturbed, and got the other one much less than half off. This was a sure sign of "very good."

Professor Ramus was a most excellent teacher of mathematics; I enjoyed his lectures, though his free use of the sponge in the left and the chalk in the right hand was greatly bewildering to the bulk of the class in the reduction and transformation of formulæ. I was just enraptured.

During the summer the scourge of cholera developed in Copenhagen, finding several victims in the families I was staying with.

One day, in June, I was invited to take dinner at the home of the director of the Polytechnic School, Professor Forchhammer, in the University Building on Nörregade and facing the Petrikirke.

During the dinner, a magnificent portrait on the wall back of the Professor and to my right hand, attracted my very special attention, so as to finally lead me to inquire whom it represented.

Almost reverentially Professor Forchhammer answered: "That is a portrait of the greatest chemist of the world, of "Berzelius."

To me, Forchhammer represented the highest type of man, as to position and learning; his naming Berzelius in that way, convinced me that he was naming one of the greatest of earth.

After half a century spent in the study of chemistry, I have more and more realized the truth of the words of my teacher of chemistry.

F. Other Processes.

We have considered with a reasonable degree of detail the ignition of the carbonate, the production of the oxide (wet way) and especially the reduction of the oxide in the dry way, all effected by Berzelius.

This was done, first to become acquainted with this new kind of work, and second, because the record of the great experimental labors of Berzelius forms the basis of our present investigation, and in truth of all serious work on atomic weights.

While it would be most interesting as well as highly instructive, to continue our exposition of the experimental data with the same amount of detail, space will not allow us to do so.

Besides, we can now readily comprehend the data in a compressed, tabular form, followed by a brief mention of the most important points involved.

In a subsequent part of this book, all the researches made will be given with a sufficient and uniform fullness of detail.

Referring to that part of this book for such details, we here shall give only the final results of all the researches made on the atomic weight of lead by Berzelius and other chemists.

In the table now following we have stated all the eleven chemical processes that have been used for the determination of this atomic weight, reserving to each one a single line.

For each one of these chemical reactions we give *first* the chemical formula of the two substances weighed, one of which has been converted into the other; *second,* we give the standard atomic weight of each of these compounds by summing the standard atomic weights of the symbols specified in the formula; *third*, we give the *atomic ratio* by dividing the second into the first, carrying out this division uniformly to five decimals. Lastly we add the *change* which this ratio undergoes if we raise the atomic weight of lead to 207.1.

The reason why we carry out the division to *five* places has already been stated, but may be repeated here. By an extended critical examination of all the atomic weight determinations we have found this number of decimals practically the limit of accuracy or precision attained.

The unit in the last or *fifth* place represents the one hundred thousandth part, by weight, of the amount of substance operated upon. This is the limit of precision attained in the best work.

Atomic Ratios for Reactions Used.

				Change.
1.	PbO	: PbO_3C	= 223 : 267 = 0.83 521	6 high.
2.	Pb	: PbO	= 207 : 223 = 0.92 825	3 high.
3.	PbO	: Pb	= 223 : 207 = 1.07 730	6 high.
4.	PbO_4S	: PbO	= 303 : 223 = 1.35 874	16 low.
5.	PbO	: $Pb(O_3N)_2$	= 223 : 331 = 0.67 371	11 low.
6.	PbO_4S	: Pb	= 303 : 207 = 1.46 377	23 low.
7.	$Pb(O_3N)_2$	: Pb	= 331 : 207 = 1.59 903	29 low.
8.	$PbCl_2$	: Pb	= 278 : 207 = 1.34 300	17 low.
9.	$Pb(O_3N)_2$	: PbO_4S	= 331 : 303 = 1.09 241	3 low.
10.	$PbCl_2$	: 2Ag	= 278 : 216 = 1.28 703	17 high.
11.	2AgCl	: $PbCl_2$	= 287 : 278 = 1.03 237	37 low.

The *change* is the number of units in fifth place changed in the direction stated by raising the atomic weight 0.1 or 207.1.

The first three reactions here tabulated have been fully considered in preceding sections.

The *fourth* reaction represents the synthesis of lead sulphate from the oxide and sulphuric acid, driving off the excess of acid by heat. It is evidently a reaction that cannot admit of a high degree of precision.

Only Turner has, in 1833, made determinations of this kind, 7 in number; range 210; mean 70 high.

The range being three times the extent of the deviation of the mean from the atomic ratio, this process is thereby proved unfit for accurate atomic weight determination.

It would surely be scientifically incorrect to ascribe the deviation to the atomic weight, while it is evidently due to the lack of precision of the reaction itself.

The change of "16 low" for a rise 0.1 in the atomic weight of lead would indicate the atomic weight 206.6, if the process could be used for such determination.

The *fifth* reaction represents the process of ignition of the nitrate. It was carried out 4 times by Anderson in Svanberg's laboratory. Range 11, mean 32 high.

This would require $Pb = 206.7$ if the process were reliable.

The *sixth* reaction represents the production of the sulphate from the metal and the acid.

Berzelius made 4 determinations. Range 78, mean 42 high.

Turner, 1833, made 3 determinations. Range 55, mean 24 high.

While in these two series of determinations we notice an approach to the atomic ratio, the range is diminishing, we notice in the later determinations by Stas, 6 in number, a diminution of the range to 24 with an increase of the deviation to 51 high, corresponding to $Pb = 206.8$.

But at the same time a mere inspection of the individual analytical ratios shows that they systematically change with the amount of lead operated upon.

Figure 2 on Plate I, facing page 31 of our True Atomic Weights, 1894, shows this fact plainly to the eye. We may also refer to the figure given by us, page 432, T. 116, of the Comptes Rendus for 1893.

There is absolutely no possibility of denying the fact, that the work of Stas gives analytical ratios systematically varying with the amount of lead used.

This fact excludes these determinations as chemically unfit.

The *seventh* reaction represents the so-called synthesis of lead nitrate from the metal and the acid. Only Stas has carried out this operation.

His series A comprises 6 determinations. Range 14, mean 71 high.

His later series B comprises 4 determinations. Range 11, mean 67 high.

Also these analytical ratios are not nearly constant with non-systematic, irregular differences, but they show systematic variations with the amount of lead used.

These systematic variations are even more marked than those shown by the sulphate. In the places just cited diagrams drawn to scale present this fact to the eye.

It is absolutely impossible to make use of any such work for atomic weight determination, because it does not even comply with the condition insisted upon in all good analytical work. See pp. 53–54.

It is in no sense our business to show how Stas came to make such a mess of this work that has been so much admired until we showed this fatal error which totally and for ever must exclude this work of his from consideration.

We tried, in our True Atomic Weights, to point out the reason. But it seems not yet to have been understood by the admirers and imitators of the work of Stas.

In this place we state once more, that this work of Stas, bearing on its face the plain systematic error (whatever its cause may be) is by this simple fact necessarily excluded from consideration in the determination of the atomic weight of lead by every chemist who understands that the amount of nitrate per gramme of metal must not systematically vary with the amount of metal employed.

If any individual, claiming to be a chemist, or even holding a professorship in chemistry and receiving a salary as such from a state or an institution, fails to understand this fact here stated and shown to exist in the record of Stas himself, such individual eo ipso is not a chemist, is unfit for such a professorship and draws the salary as an impostor.

If he be an editor of Chemical News, he is blocking the way of chemistry, and defrauding his subscribers, as is Mr. Wm. Crookes, Chemical News, vol. 73, p. 231; 1896.

The *eighth* reaction represents the synthesis of lead chloride in the dry way from the elements. The reaction cannot give reliable results for many reasons. The determinations made must necessarily be without value.

Marignac is the only chemist who has used this process. In 1846 he made 3 determinations. The range was moderate, 35; but the mean is 109 low. This would require Pb = 207.6 at least, as the change per 0.1 is 17 low.

This is not only absolutely inconsistent with the determinations of Berzelius, but conflicts also with the "demands" of the sulphate and nitrate, which would require an atomic weight considerably below 207.

The only thing to be done is to throw out and disregard faulty processes.

Our chemical record of atomic weight determinations should cease to be a stinking olla podrida, filled by the use of bad analytical methods.

The *ninth* reaction represents the change of nitrate, effected by heating it carefully with an excess of sulphuric acid.

This is a very questionable operation for atomic weight determination. Only Turner, in 1833, tried it three times. The range was small, 12; the mean was 66 high.

By our table we see that this would require the atomic weight of lead to be taken at 204.8.

It is preposterous to consider such a result seriously.

The *tenth* reaction is the unreliable silver process in the wet way.

Marignac, in 1858, made 4 determinations with a range of 101 the mean was 18 high. Dumas, in 1860, gave a mean 47 high.

These results would again raise the atomic weight, Marignac about to 207.1, Dumas to 207.3.

A blunderbuss is a rather poor thing to use where a good rifle is required.

The *eleventh* reaction is much worse, since it involves the use of silver chloride in the wet way.

Marignac made 3 determinations in 1846. The range was enormous, 380; the mean 27 low.

This mean would raise the atomic weight to 207.1 only; but the mean of such a series has absolutely no value. The range is too dangerous for the bystanders.

G. What Shall be Done with Faulty Methods and False Results?

In conclusion we find that the first three reactions, in the hands of Berzelius, gave perfectly concordant results, exactly conform to the standard atomic weight, which thus was experimentally demonstrated to be the true atomic weight of lead. These are the best and sharpest reactions, especially the third.

All the other reactions are unfit for atomic weight determinations and give conflicting results.

The ninth reaction would run down the atomic weight of lead to 204.8 while the eighth would run it up to 207.6.

This range of 2.8 in the resulting atomic weight is perfectly preposterous; it does not leave the real atomic weight in the slightest doubt, but merely confirms the opinion formed theoretically from the chemical character of the reaction, that it is unfit for the purpose.

In each one of these cases we have given striking facts showing that the reaction is unfit for atomic weight determination, either by excessive range of the results, by systematic variation with the amount of substance used, or for other reasons.

Now, what do good and *honest chemists* do when a reaction is proposed for ordinary quantitative chemical analysis?

Do they not first *test* it upon materials of known composition? and if it fails to give correct results, do they use the process when they have an analysis to make for any body? Do our treatises continue to give details about such processes, or do they at most mention and condemn them as unfit for use?

And why should any chemist act differently towards processes proposed to be used for the highest problem of the science?

If any process or reaction is proposed for atomic weight determination, and upon trial has given absurd results in conflict with rational and exact methods, or if it gives results which in themselves by an excessive range or by systematic variations or in any other way show the process to be unfit for such a purpose, is not the only thing left to be done, the exclusion of the same, and of its results, from the body of the science—with a simple note of the fact?

To suppose for a moment that by any hocus-pocus of a mathematical or enigmatical character any person can throw notoriously false results obtained by irrational process, together with such condemned by their own originators as false and worthless, into a pot or mill and turn some mechanical crank and draw out true and reliable results—is giving an exhibition of the worst possible characters of a a scientific crank.

The mere fact that such a scientific crank is the Chief Chemist of the U. S. Department of the Interior has no bearing on the chemical question involved.

And to offer to the scientific public a collection of false methods and false data obtained thereby in the garb of truth and in the form of scientific language and formula, is a crime against nature and against scientific morals.

And the institution founded for the increase and diffusion of knowledge among men per orbem, that would print, publish and disseminate such rotten abominations needs first of all a thorough driving out of the guilty, followed by a most thorough disinfection and renewal of the entire institution.

III. THE ATOMIC WEIGHT OF IRON. SVANBERG.

The determination of the true atomic weight of iron we also owe to Berzelius and his school.

With that true insight into the conditions of quantitative accuracy of chemical processes, Berzelius already in 1809, had selected the very best process possible for iron, namely, the change of the metal into its sesquioxide and the reduction of the latter.

In his Annual Report presented to the Swedish Academy of Sciences on the last of March, 1844, he gives an interesting summary of the work done by him and his school up to that time, including the final work of Svanberg.

I possess only the 8 volumes of these famous yearly reports of Berzelius, translated by Plantamour. The above historic sketch is found on pages 64 to 67 of the 5th volume of this French series, corresponding to the 24th volume of the German edition.

The earliest determination of Berzelius dates back to 1809, when he found 69.34 per cent of iron in ferric oxide by preparing ferric oxide from purest iron nails, in which he had determined the trace of carbon.

He admits, in this report, that at the time he could not suspect the effect of a small amount of silicon present in that iron.

Magnus—of the school of Berzelius—confirmed this result by reduction of this oxide in a current of hydrogen. He obtained 69.329 per cent of iron.

Stromeyer, the discoverer of the metal cadmium, showed that these results were considerably too low; but his value 69.85 found no immediate acceptance.

Hence Stromeyer, in 1843, caused the work to be carefully extended in his laboratory by Wackenroder who found, in five determinations, from 69.62 to 69.99 per cent of iron by reduction in a current of hydrogen. See also Sebelien, p. 184.

These determinations running up to within one hundredth of one per cent to the full seventy (which we here shall find to be the true value), influenced Berzelius to resume the work.

He induced *Lars F. Svanberg* to undertake a fundamental revision of the atomic weight of iron in his laboratory. This was done with the assistance of *Norlin* and "proved "that the results of Stromeyer came nearer the truth than "had been supposed."

Svanberg and Norlin, in the laboratory of Berzelius, and under his direction and with his assistance, produced a work that is worthy of the master himself.

We would like to enter upon some of the more interesting chemical points, but space forbids. We shall have to confine ourselves to the systematic statement of the results obtained, using the form already familiar to the reader.

The results obtained by other chemists will also be stated in the same form. Erdmann and Marchand are thorough representatives of the method of Berzelius.

$Fe_2 : Fe_2O_3 = 112 : 160 = 0.70\ 000$. Change 19 high.

Berzelius, 1809,				Mean	66	low.
Magnus,				"	68	low.
Stromeyer, 1826,				"	15	low.
Wackenroder, 1843,				Results from 38 to	1	low.
Svanberg and Norlin, 1844:						
Oxidation,	7 Det.,	977 — 928;	49.	Mean	47	low.
Reduction,	7 Det.,	072 — 014;	58.	"	35	high.
Mean,	14 Det.,	072 — 928;	144.	"	6	low.
Erdmann and Marchand, 1844. Reduction only:						
Substance A,	5 Det.,	030 — 962;	68.	Mean	8	low.
Substance B,	3 Det.,	055 — 015;	40.	"	38	high.
Mean,	8 Det.,			"	9	high.
Berzelius, 1844,	2 Det.,	022 — 018;	4.	"	20	high.
Maumené, 1850,	6 Det.,	010 — 990;	20.	"	1	high.

The last two series were made by wet way synthesis—dissolving pure iron in nitric acid with final ignition.

We notice that the earlier determinations were low. Berzelius over 60 low in 1809, Stromeyer only 15 low in 1826, while Wackenroder in 1843, almost reached the standard as a limit.

The most complete work of Svanberg and Norlin gave constantly low results by oxidation, high results by reduction; indicating minute constant errors acting in opposite directions, and giving the more reliable mean only 6 low. This makes their *mean* doubly valuable, according to the old rule of Berzelius.—Sebelien p. 13; True Atomic Weights p. 16—p. 3, Supra.

The determinations of Erdmann and Marchand were all made by reduction only, but the substance operated upon was obtained from ferrous oxalate prepared in two different ways.

In their results we have quite an indication of the effect of the manner of preparation of apparently the same oxide. The mean of all is only 9 high.

Berzelius himself (l. c.) accepts the results of Svanberg and Norlin with the distinct statement that his own determinations last given are not to be considered.

The final mean of the determinations of Svanberg and Norlin would lower the atomic weight about 0.03.

The mean of Erdmann and Marchand would raise it about 0.05.

But both combined would give a final mean ½ low, which would lower the atomic weight only 0.002.

Evidently, all such calculations are based upon assuming an accuracy of the mean higher than the facts substantiate.

All we can conclude is that the determinations made do not establish any deviation from the standard value 56.

Therefore, the only statement that expresses the actual experimental determinations made is that the true atomic weight of iron is 56 exactly, no positive evidence having been obtained to establish any deviation however slight, from this standard number, *not even to the extent of one thousandth of a unit.*

Before closing this subject we may mention the determinations by Richards and Baxter, recently made by reduction with *electrolytic* hydrogen.

The ferric oxide was, for the first series, obtained by calcining the hydrate; for the second series by ignition of the nitrate. The results are:

Richards and Baxter, 1900*:

Series I, 2 Det., 968 — 955; 13 Mean, 39 low.
" II, 5 " 959 — 951; 8 " 44 low.
Mean 7 " 968 — 951; 17 " 42 low.

The weighings are, of course, *stated* to the hundredth of the milligramme.

I do not see that these new determinations add anything to the stock of our knowledge.

They do not conflict with the reductions of Svanberg (mean 35 high) or Erdmann (B, 38 high).

---

* Report Chemical News, 1901, April 4; vol. 83, pp. 161 162.

Hence, the conclusion above stated, remains the true statement of all the actual experimental determinations.

The only additional point established is this, that the pretended weighing to the hundredth of a milligramme did not add the least to the accuracy of the results obtained half a century ago by weighing to the milligramme only.

The final value Fe 55.89, given by these recent authors as based upon their new data, must be thrown into the waste basket with all the other fancied values of that kind.

In conclusion we merely mention the few useless determinations by Dumas in 1860, using ferrous and ferric chloride against silver.

In our complete alphabetical summary it will be seen that the means are 70 and 90 high, with a range of 158 and 39 in ferrous and ferric chloride respectively.

IV. THE ATOMIC WEIGHT OF MERCURY. ERDMANN.

The two German Chemists, Otto Linne Erdmann and Richard Felix Marchand have done most excellent chemical work in atomic weight determination, in perfect accord with the practice of the School of Berzelius.

They have, together, made determinations upon which we base the absolute atomic weights of mercury and of sulphur. To avoid double names, we ascribe Hg to Erdmann and S to Marchand.

They made, in 1844, five admirable distillations of mercury from its oxide in a current of carbonic acid gas.

Having referred to necessary details of this admirable chemical work before, pp. 61–63, we need here give the weighings (reduced to vacuum) and analytical ratios only:

No.	Oxide.	Metal.	Analyt. Ratio.
1	82.0079	75.9347	0.92 594
2	51.0320	47.2538	0.92 597
3	84.4996	78.2501	0.92 604
4	44.6283	41.3285	0.92 606
5	118.4066	109.6408	0.92 597
Grammes.			Mean 0.92 600

Hg : Hg O = 200 : 216 = 0.92 593. Chg. 3 high.

The chemical process here expressed in standard atomic weights gives the atomic ratio stated.

The individual determinations are all high, but in the order of record only 1, 4, 11, 13, 4 high in fifth place.

The mean is only 7 high; the range only 12, the extremes being 606 — 594.

As the range 12 includes the deviation 7, the data of the experiments do not allow to depend upon this mean deviation. Furthermore, while the individual deviations are all high, the smallest brings the result to *within a single unit* in the fifth place.

The experimental data do not establish any deviation from the standard atomic weight 200, which therefore is the true atomic weight of mercury.

If we were to follow the erroneous process of calculating atomic weights to decimals not determined by the precision of the experiments, the mean would give us 200.2; but the range 12 corresponds to an uncertainty of 0.4, and thus shows the fallacy of such calculation.

V. THE ATOMIC WEIGHT OF SULPHUR. MARCHAND.

Erdmann and Marchand also distilled mercury from pure mercuric sulphide mixed with copper. The following are the weights and the analytical ratios:

No.	Sulphide.	Metal.	Analyt. Ratio.
1	34.3568	29.6207	0.86 215
2	24.8278	21.40295	0.86 206
3	37.2177	32.08416	0.86 207
4	80.7641	69.6372	0.86 223
Grammes.			Mean 0.86 213

Hg : Hg S = 200 : 232 = 0.86 207. Chg. 7 high.

The chemical process here expressed in standard atomic weights gives the atomic ratio stated. For one tenth added to the 200 of mercury, the atomic ratio would rise 7 or be 7 high.

The mean analytical ratio is only 6 high. The extremes are 223 — 206, giving a range of 17.

The individual deviations of the analytical ratios from the atomic ratio are, in the order of the record, 8 high, 1 low, zero, 16 high.

The middle two determinations are exactly coincident with the calculated value. The first deviates to about one tenth and the last to about two tenths of unit on the atomic weight of mercury, raising the same.

But the range or uncertainty of 17 (corresponding to 2½ tenths) is greater than these deviations

Accordingly, we are compelled to admit that the experimental determinations fix the true atomic weight of mercury at the value of its standard atomic weight, within the limit of precision of the determinations made.

But the work on the oxide fully establishes the atomic weight of mercury as 200. We can, therefore, use these distillations of the sulphide for the determination of the atomic weight of sulphur, precisely as originally intended by these eminent chemists.

To do so, we need only calculate the change in the atomic ratio corresponding to a rise of 0.1 in the standard atomic weight of sulphur, namely 32.

We find this change (for S) *37 low.* Now, the mean analytical excess was found above to be 6 high. This is a trifle less than $\frac{1}{6}$ of the change; hence corresponds to a departure of $\frac{1}{6}$ of one tenth or 0.017, direction low.

Hence $S = 31.983$.

We understand, of course, the true signification of this expression. It means that the determinations of Erdmann and Marchand give a possible departure of 0.02 low of the true atomic weight of sulphur 32, but that this departure is *not* established, rather simply marks the limit of precision.

VI. THE ATOMIC WEIGHT OF CHLORINE. TURNER.

The atomic weight of mercury having been established, we can next use other mercury compounds for the determination of other metalloids.

Since mercuric chloride can be produced in purest crystal form, its distillation will furnish the atomic weight of chlorine.

Lars Svanberg made three excellent distillations with lime according to the method of Erdmann and Marchand.

$Hg : HgCl_2 = 200 : 271 = 0.73\ 801$. Change 48 low.

Svanberg's Distillations, 1848.

No.	Sublimate.	Mercury.	Analyt. Ratio.	Excess.
1	12.048	8.889	0.73 780	21 low.
2	12.529	9.2456	794	7 low.
3	12.6491	9.3363	810	9 high.
Grammes.			Mean 0.73 795	6 low.

We notice, the deviations are to both sides, very small, except the first. If this first attempt were discarded, the final excess would be 1 high only.

Taking all determinations as of equal value, the mean analytical excess is 6 low, which represents ⅛ of the change due to 0.1, or 0.012 on the atomic weight, in opposite direction, hence giving a positive departure.

That is 35.512 or say 35.51.

The real meaning is that 35.5 is the true atomic weight, with a *possible* deviation indicated of 0.01 high, but not fixed, as it is within the limit of precision.

Millon, in 1846, had made four less accurate distillations, obtaining a mean analytical ratio of 0.73 845 which is 44 high.

Turner, in 1833, set free mercury by means of stannous chloride, collecting and weighing the mercury thus set free. His results are:

No.	Sublimate.	Mercury.	Analyt. Ratio.	Excess.
1	60.682	44.782	0.73 798	3 low.
2	99.06	73.09	784	17 low.
Grains.			Mean 0.73 791	10 low.

The work of Svanberg in 1848, is considerably more accurate than that of Turner in 1833, as is but natural, especially as Svanberg had the benefit of the excellent work of Erdmann and Marchand.

If Svanberg's name were not already associated with iron, it would belong here for chlorine.

Consequently the honor belongs to the best chemists of the earlier schools; that is, to Turner, who has done so much really good work in this field.

We may find this decision objected to, at first sight. But since Turner established the atomic weight of chlorine within the limit of 0.02 while thirty years later Stas was over 0.05 in the wrong direction, I suppose the honor of Turner will not be contested.

Let us see. The mean, 10 low, with change 48 low, is practically one-fifth of one-tenth, or 0.02 and high, or $Cl = 35.52$. That is, as above stated, establishing the absolute atomic weight at 35.5 exactly, within 0.02 as the limit of precision, preferably upward.

Good Old Chemists Abused by Clarke.

In conclusion I have once more to refer to those "Constants of Nature" because, as usual, Clarke is shamefully unjust to our excellent pioneer workers.

The very first sentence under Mercury (edition 1897, p. 166) reads:

"In dealing with the atomic weight of mercury we may
"reject the early determinations of Sefström and a large
"part of the work done by Turner."

Sefström's work dates from 1812, and reaches to 7.97 for oxygen per hundred of mercury; that is within 0.03 of the truth. It corresponds to $O = 15.94$; is therefore twice as accurate, as "the latest fad of Clarke, 15.88."

Turner, in two determinations of pure oxide (from nitrate) obtained the analytical ratio 0.92 605 which is 12 high only. His determination of chlorine is much more correct than that of Stas.

Both of these early chemists did most admirable work, indeed. They deserve our highest respect. Their work is more reliable than much of the work of to-day.

The chief official chemist of our National Government ought not to defame the great early chemists who did excellent work in atomic weight determination—a work that he has disgraced.

Hardin's Electrolyses.

His electrolyses of mercuric oxide have been withdrawn. See p. 30, supra.

Those of silver and mercury compounds in the same circuit, have also been destroyed by "selection;" see Hardin's Thesis, 1896, pp. 38, 39.

There remain his electrolyses of the chloride, bromide and cyanide of mercury, published in that Thesis.

There also remains an ugly suspicion of selection, of course. His results are remarkably concordant, and support the Stasian values of Clarke—which need all support they can get from any quarter.

But surely, no one can accuse this hopeful young Stasian to have "selected" anything in favor of our heterodox atomic weights. Let us therefore, examine the three series he has not withdrawn, after publication.

He always weighs the substance and the metal. We will state his results in three lines, giving our *atomic ratio first:*

Atomic Ratio.	Comp'd.	Analyt. Results.
0.55 555	Bromide.	565 — 548; 17. Mean 0.
0.73 801	Chloride.	838 — 820; 18. Mean 28 high.
0.79 365	Cyanide.	342 — 337; 5. Mean 26 low.

Taking the entire set of 10 determinations each for these three compounds, bona fide, they confirm our Hg = 200 exactly, in a most remarkable manner.

For while the mean analytical excess for the bromide is zero, those for the other two compounds almost exactly balance.

The mean analytical excess of all thirty determinations is practically zero. Our standard atomic weight is also the absolute, true atomic weight, Hg = 200 *exactly*, according to the 30 experiments of Hardin, if they are bona fide determinations.

If so, we have an interesting case of constant errors determined by the nature of the substance operated upon. For the bromide, as might be expected, the constant error is zero; for the other two it operates in opposite directions, balancing in amount.

The case of Hardin shows exactly how demoralizing the influence of the Chief Chemist Clarke has been. Concordance, minute "probable errors" are insisted upon by this High Muckamuck of the National Government.

Hence—the supply follows the demand; and with it, truth and science are defaced, and a probably excellent young worker wrecked.

How long is this nation going to allow our official Olla Podrida Cook to terrorize American students of Science and to disgrace American chemistry?

VII. THE ATOMIC WEIGHT OF CARBON. DUMAS.

The great work done by Dumas in perfecting the process of the quantitatively accurate combustion of the diamond was strictly in line of the school of Berzelius, though published as an attack on the Swedish chemist.

It was soon followed by the perfection of the process devised by Berzelius for the determination of the atomic weight of hydrogen. In his later years, Dumas unfortunately made use of the method of Pelouze and furnished inaccurate data in great number.

Having given (p. 39) all the experimental data of the five combustions of Diamond, by Dumas, we need here only present a summary of the results of all such combustions published up to the present date.

This reaction is atomically one of the most sensitive in chemistry, so that *four decimals* here will give a much higher degree of precision than five commonly used.

$CO_2 : C = 44 : 12 = 3.66\,667$. Change 2204 low.

To 4 decimals: 3.66 67. Change 220 low.

Accordingly, we shall state all results to *four decimals only*, for carbon. The ratio is exactly 11 to 3.

We shall also add the total weight in grammes, of diamond burnt by each analyst.

Dumas and Stas, 1840:
5.40 gr., 5 Det., Extr. 95 — 28; 67. Mean 2 low.
Erdmann and Marchand, 1841:
4.83 gr., 5 Det., Extr. 73 — 96; 77. Mean 30 low.
Roscoe, 1883:
6.03 gr.. 5 Det., Extr. 75 — 49; 26. Mean 5 low.
Friedel, 1884:
1.33 gr., 2 Det., Extr. 40 — 28; 12. Mean 33 low.
Mean of first 3 sets of 5 det. each, 12 low.

Total weight of diamond burnt 16.26 grammes in these 15 determinations, averaging 1.08 grammes in each.

The four series of determinations divide sharply into two groups, according to the amount of the analytical excesses. To obtain the corresponding effect on the atomic weight, we must remember that a rise of 0.1 corresponds to 220 low in the fourth place.

Hence Dumas mean, 2 low, corresponds to 12.001; that of Roscoe to 12.0025.

The second group, giving an analytical excess of about "30 low" corresponds to about 12.017.

Since Dumas and Roscoe used over 11 grammes of diamond against the others only about half as much, it is evident that the former had the best chance of getting accurate results.

It will be noted, that Friedel had only about half a gramme for each determination, while all the others averaged a gramme for each determination.

We must conclude that the atomic weight of carbon (diamond) is 12 exactly, within the limit of the errors of the experiment.

This limit is 0.001 in the case of Dumas, 0.002 in the case of Roscoe, and 0.017 in the case of Erdmann and Marchand, and for Friedel also.

This is the simple record of the facts ascertained. It is most admirable.

A False Correction.

Recently A. Scott has called attention to the effect of the absorption of carbon dioxide on the volume of the saturated potassium hydrate. Journal Chemical Society 1897, pp. 550–564.

Taking all the determinations made for "*carbon*" this chemist now applies an additional correction, and finds as final "mean value" C = 12.0008, which even Ostwald concedes to bring the deviation from the "round number" 12 into the region of the errors of experiment. Ztsch. 24, p. 377; 1897.

This apparent "correction" has probably induced the Three German Chemists (Ostwald, Landolt and Seubert) to put C = 12.00 in the table of the atomic weights adopted by the German Chemical Society in 1898.

But we most respectfully beg to object to this correction made en-bloc for "*carbon*" and for this additional muddling the atomic weight of carbon and any such "rounding off" to 12.00.

In the *first* place, we deem all such minute "corrections" applied half a century after the publication of great standard determinations of very doubtful propriety. There is another gnat strained at, and another drove of camels to be swallowed.

If an error has been committed, let the "correction" be made on *new* work, but leave the results of the old masters "uncorrected" and undisturbed.

In the second place it is *not correct* to apply any correction to combinations of *all sorts of so-called carbon* for the purpose of establishing the atomic weight of carbon. Here comes that drove of camels, longing to be swallowed.

Not even natural graphite can be used for this purpose, and the application of artificial graphite, is out of the question. Already Dumas declared that graphite could not be weighed with absolute precision. Compare p. 48.

To include the combustions of "*sugar coal*" and "*paper coal*" of Van der Plaats, 1885, in the experimental data for the determination of the atomic weight of carbon is chemical folly.

To apply "corrections" to work undertaken with material which is not fit to be weighed with precision, is absurd.

It is true that Dumas made determinations with graphite as well as with the diamond.

But the atomic weight which he adopts is the one determined by means of the diamond *only*.

To prove this we need only tabulate the determinations made by Dumas (and Stas) in 1840, in our usual form (for four places only).

We shall add all other combustions in the same form.

Combustions of Different Sorts of Carbons.

Dumas and Stas:					
Diamond,	5 Det.,	695 — 628;	67	Mean	2 low.
Graphite, Nat'l,	5 Det.,	710 — 670;	40	“	16 high.
Graphite, Artif.,	4 Det.,	744 — 654;	90	“	32 high.
Erdmann and Marchand:					
Diamond,	5 Det.,	673 — 596;	77	Mean	30 low.
Graphite, Nat'l,	3 Det.,	647 — 609;	38	“	29 low.
Graphite, Artif.,	1 Det.,			“	39 low.
Roscoe:					
Diamond,	5 Det.,	675 — 649;	26	Mean	5 low.
Carbonado	1 Det.,			“	55 low.
Van der Plaats:					
Graphite,	3 Det.,	664 — 663;	1	Mean	3 low.
Sugar Coal,	2 Det.,	660 — 655;	5	“	10 low.
Paper Coal,	1 Det.,			“	10 low.

I deem it superfluous to add many words to this striking tabulation of the record.

The figures—*giving a total range of the mean, nearly one hundred*—utterly condemn any combination of all these determinations when the object is the determination of the atomic weight of true carbon.

For the *first* condition in such a problem is to use the purest material possible; that is the diamond, which by its very physical and chemical properties can be handled and cleaned, as Dumas already accentuated.

Even graphite “natural” can *not* take the place of the diamond for this purpose—as also plainly implied in words by Dumas half a century ago. Compare p. 48, supra.

We cannot get a general mean of all determinations made for carbon, by including any determinations of Van der Plaats, because he made none on purest carbon, the diamond.

It is perfectly in order to find all determinations made on *any sort of carbon* treated as one, individually affected by "probable errors" only, in the *chemical olla podrida* of Clarke, furnished by the Smithsonian Institution of Washington.

But it is too bad for a good English Chemist to apply a minute correction to all such determinations indiscriminately and then give us a corrected atomic weight of carbon to the fourth decimal place.

This correction, applied to all sorts of carbon, and the "corrected" result C = 12.0008 copied into Ostwald's Zeitschrift is really more than even my tolerant nature could stand.

May we not expect that the chemists of to-day will use a modicum of common sense when handling the subject of atomic weights?

After a patient and careful consideration of all experimental determinations made with the purest material and by the most sensitive method of Dumas, I am convinced that the atomic weight of true carbon does not differ by as much as one thousandth of a unit from the exact number 12, the atomic weight of oxygen being taken at 16 exactly.

As a most characteristic chemical curiosity I translate from page 85 of Ostwald's Physik. Chemie, Bd. I, 1891, the following:

"There can remain no doubt but the atomic weight of
" carbon is to that of oxygen as 12 to 16, *within the errors of*
" *the experiments,* and which may amount to a few ten-
" thousandths of the total value. We use the value
" C = 12.003."

First declare it is 12 exactly, within the minute errors of the experiment; then use a *different* value throughout the work, thus known to be wrong.—"Es muss auch solche Käutze geben."

VIII. THE ATOMIC WEIGHT OF CALCIUM.

The best determinations have been obtained by using purest calcite, Iceland Spar, as substance.

The first determinations, made by Dumas, give a mean analytical excess of 55, which would correspond to the atomic weight C = 40.055.

The determinations by Erdmann and Marchand on spar are very fine, giving a mean analytical excess of 28, corresponding to Ca = 40.028. Their determinations on artificial carbonate bring the mean excess almost to zero, and the atomic weight almost to 40 exactly.

On account of the high importance of these determinations we reprint the weighings from our True Atomic Weights, p. 184, which were copied from vol. 8 of the Annales de Chimie et de Physique for 1843.

In the work of Clarke, which at least ought to give the data of observations in full, these data are horribly incomplete. Fortunately, the volume of the Annales was at the Mercantile Library of St. Louis.

$CaO : CaO_3C = 56 : 100 = 0.56\ 000$. Change 100 high.

Dumas, 1842. Dissociation of Iceland Spar:

	Weight in Grammes.		Analytical	
No.	Spar.	Residue.	Ratio.	Excess.
1	49.916	28.016	0.56 123	93 high.
2	50.497	28.305	053	23 high.
3	64.508	36.167	066	38 high.
		Due impurity,	030	

Annales de Chimie et de Physique, III Series, T. 8, p. 202.

This excess would correspond to Ca 40,023 to 40.093; mean 40.051.

Erdmann and Marchand, 1842. Artificial Carbonate:

1	8.2335	4.6135	0.56 033	33 high.
2	10.8850	6.0940	0.55 985	15 low.
3	10.1315	5.6740	0.56 004	4 high.
4	5.5310	3.0970	0.55 994	6 low.
		Mean	0.56 004	4 high.

Corresponding atomic weight Ca = 40.004. Same volume of Annales, p. 14.

Erdmann and Marchand, 1844. Iceland Spar.

1	4.2134	2.3594	0.55 997	3 low.
2	15.1385	8.4810	0.56 022	22 high.
3	23.5503	13.1958	0.56 031	31 high.
4	23.6390	13.2456	0.56 032	32 high.
5	42.0295	23.5533	0.56 044	44 high.
6	49.7007	27.8536	0.56 042	42 high.
		Mean	0,56 028	28 high.
	1850, 1 Det., Artif. Carb.,		0.55 998	2 low.

This last case made with utmost care. Combined with the mean of the four determinations on artificial carbonate, the general mean would be 1 high, 1 corresponding to 40.01.

$Ca O_4 S : Ca O_3 C = 136 : 100 = 1.36\ 000$. Change 36 low.

Erdmann and Marchand, 1842. (Annales T. 8, p. 216):

	Weight in Grammes.		Analytical	
No.	Spar.	Sulphate.	Ratio.	Excess.
1	2.370	3.225	1.36 076	76 high.
2	4.796	6.5255	061	61 high.
3	3.065	4.1690	020	20 high.
4	5.446	7.4100	063	63 high.
		Mean	1.36 055	55 high.

This mean would correspond to 39.85.

For Ca = 40, change to S = 32.1 would give 100 high.

Hence, 55 high would correspond to S = 32.055.

$$Ca Cl_2 : Ag_2 = 111 : 216 = 0.51\ 389.$$

Dumas, 1859. Volumetric.

2 — 3 gr. Det. 5, Extr. 573 — 394; 179. Mean 66 high.

Since for 40.1 the ratio would be 46 high, this mean would correspond to Ca = 40.14, while the range 179 correspond to a range of 0.4 or one per cent of the total atomic weight.

I understand that T. W. Richards has also made determinations, probably on the bromide, as he is wont to do; compare his work for Mg, Sr, Ba, Zn. It is all of the same general character.

The results obtained by the dry and the wet way differ in accuracy as was stated in an earlier section, pp. 46–55.

The dry way, almost exclusively used by Berzelius, is the only way to employ if the element concerned permits it.

The final result is that the atomic weight, within the precision of the determinations, is 40 exactly. The experimental determinations show that the actual atomic weight does not differ as much as 0.01 from the standard.

IX. THE ATOMIC WEIGHT OF MAGNESIUM. SCHEERER.

Marchand and Scheerer, in 1850, selected the purest natural magnesites for the determination of the atomic weight of magnesium by the direct dry way process, used so effectively for calcium.

They selected three very fine varieties of this mineral. A yellow, transparent magnesite from Snarum; a white, opaque variety from the same locality and a very pure, but opaque, white variety from Frankenstein.

Chemical examinations, made with extreme care, by Scheerer, revealed the presence of 0.00 225 of lime (Ca O) in the Frankenstein magnesite, and 0.00 430 of lime together with 0.00 776 of ferrous oxide in a unit of weight of the Snarum magnesite.

Scheerer uses the results from both localities; but we deem such process irrational, because the iron is likely to change its degree of oxidation, and even if it does not under the circumstances, the purer substance must always be preferred.

We, therefore, exclude all data obtained from the Snarum magnesite, also the first two series of determinations made with Frankenstein's magnesite, and use exclusively the third series made upon the purest material of this fine variety.

Now, the 225 of Ca O found require 177 CO_2 and constitute 402 of $Ca\ O_3\ C$ in the magnesite used, leaving of actual $Mg\ O_3\ C$ only 99 598 — all figures in fifth place.

The mean residue of the four determinations of this third series amounted to 47 642; since 225 was Ca O, the true Mg O amounted to 47 417 only.

But this in the 99 598 of pure magnesium carbonate amounts to 0.47 608 per unit.

This value must be considered as the most reliable determination. The range of the four determinations of this third series was 50.

Now this mean value of the analytical ratio is exactly 11 low of the atomic ratio, which is

$Mg O : Mg O_3 C = 40 : 84 = 0.47\ 619$. Change 62 high.

The analytical excess being 11 low, corresponds therefore to the atomic weight 0.018 low, or say Mg = 23.982 or better 23.98.

But to state this as the true value would imply the disregard of actual errors of the chemical work.

The range of 50 represents an uncertainty of 0.08 in total range, or perhaps more fairly of 0.04 on the mean.

In the summary of Clarke (p. 140) all three series made with Frankenstein magnesite are, of course, combined, giving 0.47 628 as final mean, which is 9 high.

The Snarum magnesite gives him 0.47 624, which is 5 high.

His final mean is 0.47 627, which is 8 high.

Restricting ourselves, on principle, to the purest material used, makes our analytical ratio 0.47 608, which is 11 low.

The three series made with Frankenstein magnesite would have given 9 high, the Snarum magnesite 5 high and the mean of all 8 high.

These analytical excesses correspond respectively to a *rise* of 0.014, 0.008 and 0.013 on the atomic weight of 24.

The third series alone, made upon the purest material, gave us the analytical excess of 11 low, which would correspond to a lowering of the true atomic weight 0.018 below the standard 24.

We can therefore truly say that, taking the series we deem the most conclusive, or taking all or indeed any one, the difference between the true and standard atomic weight cannot reach 0.02 either way.

Accordingly, the dry way work on magnesite, instituted by Marchand and finished by Scheerer fully half a century ago, establishes, as a fact, by experimental work of highest order, that the true atomic weight of magnesium is *not* different from the standard atomic weight 24 within the limits of precision, which may be taken at 0.02.

Richards' Determinations.

Several other methods have been tried; but the alphabetical record in Part IV, shows that these methods were defective, either in the starting material, the final product or even in both, when dry way processes, or they had some of these defects and were made by some much less reliable wet way process.

It is not necessary here to give any of these except, perhaps, the volumetric process, rejuvenated by Richards, of Harvard.

$MgCl_2 : Ag_2 = 95 : 216 = 0.43\ 982$. Change 46 high.

Dumas, 1860:

1 — 4 gr. Det. 11, Extr. 380 — 154; 226. Mean 279 high.

Richards and Parker, 1896:

Series II, Det. 3, Extr. 152 — 130; 22. Mean 160 high.
" III, Det. 6, Extr, 144 — 131; 13. " 156 high.
" IV, Det. 6, Extr. 138 = 136; 2. " 155 high.

Evidently the entire object of the work of Richards is to obtain concordance and with it the praise of our olla podrida maker in Washington.

Richards Excels Dumas 4,000 Times.

This praise Professor Richards has received (p. 144) in the following words:

"Here the first two values" (Dumas and Series II of Richards and Parker) "practically vanish, and the third and "fourth series of Richards and Parker *appear alone*."

The reason of this high praise rests upon the "weight" of the determinations always measured by the inverse square of the probable error of the mean by Clarke, in his "Constants of Nature."

These probable errors are given in units of the fourth place of the per cent, that is, in units of the *sixth place*—the millionths of the unit of weight—according to our tables.

These “probable errors” are given by Clarke on page 144 of the American Chemical Olla Podrida as 200, 43, 13, 3 millionths per unit of weight operated upon.

The *squares* hereof are therefore 400 00, 1849, 169 and 9 units in the *12th place* or the millionth of millionths of the gramme, per gramme operated upon.

In passing, we may state that Richards and Parker generally used less than two grammes of their pure magnesium chloride (they suppose, for it cannot be weighed accurately); never as much as 3 grammes.

Taking the relative values only, and putting the skill and perfection of the work of Richards in his third series at one million, we find according to the process of this same Olla Podrida Americana, the skill of the others given as follows:

Richards and Parker,	IV Series,	1,000 000
“ “ “	III “	52 632
“ “ “	II “	4 878
Dumas, 1860 only		225

As this table of perfection is not quite readily grasped, we will reverse it.

We will put the skill and ability of the great French Chemist Dumas, as *one* and calculate the skill of the Harvard Chemists in that unit.

In this way, the above figures will present:

		Skill.
Chemist Dumas, 1860,		1
Harvard Chemists,	II Series,	17.5
“ “	III “	210
“ “	IV “	4,000

The chemists of Harvard, “Richards and Parker,” started out in this atomic weight determination twenty times more perfect in their work and methods, than was Dumas almost at the height of his fame.

Well, we may put this twenty to the credit of Modern Chemistry and America; and the modest young American Chemist may not protest. The Present and America is the pedestal on which they proudly stand.

But just note the wonderful capacity for progress in the work of these young chemists of Old Harvard.

In their third series the work is 200 times as excellent and reliable, according to the Smithsonian mode of calculation, than in their second series.

In their fourth series they have risen to the exalted standard four thousand times the capacity of Dumas in 1860, when he was considered the greatest chemist of the world.

And how rapidly our young chemists of Old Harvard develop, how astonishing the rate of their progress, how tremendous the swiftness wherewith they do "evolute!"

Starting "*only* 20 times as perfect as Dumas," they are ten times surpassing themselves in the work of the third series, and two hundred times when closing the fourth series.

And all this progress and evoluting done in the space of a few months!

Not a word is inaccurate in the above, not a figure or proposition that is not obtained *strictly according to the fundamental formulae, pp. 7 and 8 of the "Constants of Nature*," produced by Frank Wigglesworth Clarke, Chief Chemist of the Geological Survey, in the employ of the Secretary of the Interior of the United States of America, and published with the endorsement of the Secretary of the Smithsonian Institution at the cost of the fund which the Englishman Smithson gave to the United States Congress for the foundation of an "*Institution for the Increase and Diffusion of Knowledge among Men per orbem.*

Richards Really Progressed 0.01 Only.

The measure of precision has been considered seriously as it must be, since we follow the directions and formula of a Government Publication.

Now let us see how the final results look when examined as to its absolute value according to our standard.

This, our standard, has now been tested by the atomic weights of lead, iron, mercury, sulphur, chlorine, carbon and calcium, that is, by the great chemical elements, great in every sense of the word.

These atomic weights are based upon the work done by Berzelius, Svanberg (in the laboratory of Berzelius), Erdmann and Marchand, the best German chemical workers in this field, and Dumas, the greatest French representative.

Our standard exactly expresses the work of these masters under one general formula; let us now test the work of the young chemists of Harvard, who are by the Government and Smithsonian standard four thousand times better chemists than was Dumas.

We see, by the table of the analytical excess given above, that the analytical excess of Dumas was 279 high (in the fifth place); that the young chemists of Harvard started out, in their second series at about half this excess (exactly 160); that in their third series they brought this excess down only four additional units of the fifth place, and in the fourth series they diminished it only by one unit more.

The entire "Progress" they have made, measured according to our standard, is the mere reduction of the excess from 160 to 155, that is 5 units!

Now 155 high, represents 0.34 high on the atomic weight of magnesium, putting it at 24.34.

Their second series, 160 high, placed this atomic weight at 24.35.

While these young Harvard chemists, according to the Smithsonian Institution and United States Government Chief Chemist, became two hundred times more perfect in precision, they only succeeded in paring off one measly little hundredth from the atomic weight magnesium, bringing it down from 24.35 to 24.34.

But these young chemists of old Harvard, and the author of the American Olla Podrida of Official Chemistry in Washington will say "again" that my standard is not right, that only Mark Twain will take it into consideration, and what not else of abuse and denunciation they may bring forth when among themselves or at the head of some chemical fraternity. See my General Chemistry, Lect. 99, Art. 15.

I think we have given enough of data to show that our standard is fully established by the work of all the great masters in this branch of chemistry, even up to this point. We shall, in the next few chapters, greatly strengthen it by the best work done in recent days.

Scheerer and Richards.

But entirely independent of all this, let us for a moment look at the two conflicting results of Scheerer and Richards.

Let us put it in a table, using the modern deadly parallel column method which we learnt when a boy, dwelling with Robinson Crusoe on his lonely island in the sea.

	Scheerer	Richards.
Atomic Weight,	24.00	24.34
Method,	Dry Way.	Wet Way.
Process,	Ignition.	Titration.
Substance,	Magnesite.	Chloride.
Weighable,	Perfectly.	Not directly.
Product,	Magnesia.	Silver.
Weighable,	Perfectly.	Not directly.
Difference,		1½ per cent.

Finally, for this value 24.34 of Richards, the analytical excess in the dry way work of Scheerer should have been 211 high; it is preposterous to suppose that Scheerer could have been so much in error in dry way work.

Tanagra Atomic Weights.

It is not worth while saying anything more about this subject except we might mention a somewhat parallel case that has attracted the attention of the public of the United States quite recently.

At Boston, almost in the shadow of noble old Harvard, they have a Museum of Fine Arts, which contained twenty-eight costly, rare and choice Antique Greek Statuettes, known as the *Tanagra Terracottas.*

By the news dispatches in the public press of the country at the close of November, 1900, the people of this land were informed that 23 of these 28 Tanagra Terracottas had just been removed as forgeries pure and simple.

According to this, about 5 in this quarter hundred were (at least possibly) genuine, at any rate, about *twenty per cent had not been proved to be forgeries.*

It was but natural for my thoughts to flit from the Boston Museum of Fine Arts across the "Cam. Bridge" to the Chemical Laboratory of Harvard University where I visited Professor Cooke some thirty-five years ago.

How happy the authorities of Old Harvard will have reason to be if twenty per cent of the atomic weights recently manufactured in their Chemical Laboratory are not demonstrably false and fraudulent.

Postscript. While reading the proof I find, in Science for July 5, 1901 (p. 36), the following quoted from an article written by Professor Richards for the Harvard Graduate Magazine on *Research Work in Harvard Chemical Laboratory.* The italics are ours.

"In the last ten years the atomic weights of copper, "barium, strontium, calcium, zinc, magnesium, cobalt, "nickel, uranium and cesium have all been studied *with* "*a care which seems to carry conviction with it.* This work "has all been handicapped by the inadequate quarters in "which it had to be performed, and we now have to face "the bitter alternative of being obliged either to *turn away* "*graduate students,* or else so to crowd them together as "to make accurate investigation almost impossible."

It would most assuredly be best for the good repute of Harvard to have this manufacture of Tanagra Atomic Weights closed.

Ten such Tanagras in ten years is too much. Our Chemical Rumpelkammer will have to be enlarged.

It might be an admirable plan to give Professor Richards a rest to season.

"Graduate Students" at Harvard might be "turned away" into other fields of chemistry to browse, where they would do less harm to themselves and be less likely to disgrace Old Harvard.

X. THE ATOMIC WEIGHT OF PLATINUM. SEUBERT.

The most common and general reaction for platinum is the formation of the so-called double chloride with potassium or ammonium chloride. This reaction is of daily use in qualitative analysis for the detection of these metals and has been found to extend to compound radicals and even alkaloids.

The most decisive and elegant method of applying these tests in microchemistry is based upon the wonderful crystallizing tendency of these so-called double salts.

The common microchemical test for potassium or ammonium by means of the platinic chloride has all the characters of a first class test: Sensitive, decisive and beautiful.

The great value of these reactions was already recognized by Berzelius for atomic weight determinations. In one experiment he used nearly 3 grammes of platinum and obtained an analytical ratio exact to the third decimal.

This reaction has been very skillfully used by Karl Seubert for the determination of the atomic weight of platinum.

I take great satisfaction in being able to put the name of Karl Seubert at the head of this section, as that of a modern Chemist who has done chemical work truly in the spirit and according to the methods of Berzelius in so excellent a manner that I doubt not, his name will remain connected with the atomic weight of this most remarkable modern metal.

In the future we trust there will be less determinations and much better ones than have been produced since Stas demoralized chemistry and Lothar Meyer re-calculated atomic weights.

I contend that the name of the analyst at the head of each atomic weight will remain there, as surely as that of the founder of a new genus in botany.

Seubert has produced the first positive determinations permitting the establishment of the true and absolute atomic weight of platinum. That fact cannot be changed. Hence that name must stay.

These so-called double chlorides are really chloro-ternaries; see our general formulæ, also in the "Statistik der Krystall—Symmetrie" presented by Haidinger to the Academy of Sciences of Vienna, Sitzungsberichte, I Abth. Bd. 62, 1870—Typus, Tetrate; IV. Chloro-salze.

The proper names are potassium chloro-platinate, ammonium bromo-platinate, etc. We must be permitted to use these names in this section.

The only additional chemical fact which it is important to bear in mind is the ready and complete decomposition of these compounds by ignition, leaving always the platinum in the metallic state, the potassium as water-soluble chloride, while ammonium and similar radicals are completely volatilized.

It is this ready dry way decomposition leaving the product in a most admirable condition for exact weighing, ready production of the substance in a chemically pure crystal form, which first attracted the special attention of Berzelius to the simpler members of this group of salts for atomic weight determinations.

We shall now give the experimental data in our usual form.

$Pt : Am_2\ Cl_6\ Pt = 195 : 444 = 0.43\ 919$. Change 13 high.

Seubert, 1881:

Series I, Det. 6, Extr. 963—946; 17. Mean 37 high.
" II, Det. 6, " 889—871; 18. " 43 low.
" III, Det. 9, " 026—986; 40. " 82 high.
Mean of all, 21 Det. " 34 high.
" " Series I, II, 12 Det. " 3 low.

Halberstadt, 1884:

Reduction, Det. 10, Extr. 011—880; 131. Mean 32 high.
Electrolysis, Det. 8, " 979—894; 85. " 15 high.
Mean of all, 18 Det. " 24 high.

$Pt : Ka_2\ Cl_6\ Pt = 195 : 486 = 0.40\ 124$. Change 12 high.

Seubert, 1881:

4—7 gr., Det. 8, Extr. 130—070; 60. Mean 17 low.

Halberstadt, 1884:

Reduction, Det. 8, Extr. 127—069; 58.
Electrolysis, Det. 11, " 126—063; 63.
All 19 Det., 127—063; 64. Mean 26 low.

$Pt : Am_2\ Br_6\ Pt = 195 : 711 = 0.27\ 426$. Change 10 high.

Halberstadt, 1884:
Reduction, Det. 23, Extr. 465 — 390; 75.
Electrolysis, Det. 9, " 456 — 386; 70.
All Det. 32, " 465 — 386; 79. Mean 3 high.
$Pt : Ka_2 Br_6 Pt = 195 : 753 = 0.25\ 896$. Change 10 high.

Halberstadt, 1884:
Reduction, Det. 12, Extr. 957 — 880; 77.
Electrolysis, Det. 6, " 927 — 877; 50.
All Det. 18, " 957 — 877; 80. Mean 19 high.

$Pt : Pt Br_4 = 195 : 515 = 0.37\ 864$. Change 17 high.

Halberstadt, 1884:
Reduction, Det. 8, Extr. 873 — 839; 34.
Electrolysis, Det. 2, " 837 — 819; 18.
All Det. 10, " 873 — 819; 54. Mean 17 low.

The bromo-platinates used by Halberstadt give a fine confirmation of the work of Seubert. The Platinic Bromide is also valuable.

In looking at all these determinations we notice that Series III of Seubert is the only one which conflicts measurably with all the others. As the three series of ammonium chloro-platinate differ in the preparation made use of, we are inclined to suspect that the error was in a lack of purity of the substance used for Series III. Its great range also is marked. At any rate, we are fully authorized to throw it out, as a single, unexplained conflict in over a dozen series.

The determinations of Halberstadt for ammonium chloro-platinate show a much greater range than those of Seubert; the analytical excess is also greater.

The most important fact is that Seubert found the analytical excess in the first series *high* and in the second *low* to about the same extent, say about 40, corresponding to 0.3 on the atomic weight of platinum in either direction.

This is a most important indication of remarkably close work.

Both investigators find the analytical excess low for the potassium chloro-platinate, to the extent of from one to two-tenths on the atomic weight of platinum.

As for the ammonium salt this excess was generally low, we have here, in the potassium compounds, a distinct indication of minute differences of deportment working in opposite directions.

The ammonium bromo-platinate comes out practically exactly on the atomic ratio, while the potassium salt gives an analytical excess "high" or positive, corresponding to about 0.2 on the atomic weight of platinum.

It will be noticed that the chloro-salt of potassium was "low" to about the same extent.

Thus, combining such salts, differing only by one element, we notice a compensation of the minute errors or deviations.

We need not re-state that it would be mathematically absurd to calculate the atomic weight from these data taking them to be absolutely exact, and then to wonder and ponder how the chloro-salt will give a higher value for the atomic weight of platinum, than the bromo-salt, or how the ammonium compound can give a different atomic weight from the potassium compound.

The fact that the sign or direction of the analytical excess oscillates with the metal (Ka, Am) or the intermediate (Br, Cl), while the numerical amount of this analytical excess remain practically the same, shows that we here simply are at the very limit of precision of this kind of chemical work.

As now the amount of this analytical excess corresponds to at most two-tenths on the atomic weight of platinum, it is palpable, that the totality of these experimental determinations demonstrate the atomic weight of platinum to be 195 exactly, which the individual determinations or series of determinations hit squarely or deviate from in either direction to an extent not exceeding 0.2.

The most excellent chemical work done by Seubert in 1881, permits us to say that the true atomic weight of platinum does not differ from the standard value 195 by any fraction within the limits of precision attained, which is about 0.2.

All deviations from 195 actually noted, are within this limit, and about equally frequent, according to the elements, positive and intermediate, present in the platinum salt used.

XI. THE ATOMIC WEIGHT OF THALLIUM. CROOKES.

Praemonitio: Mr. William Crookes, now Sir William Crookes, as editor of the Chemical News, has, on two occasions, about a quarter of a century apart, grossly abused his position as editor against me, and by wilfully misleading his subscribers, has defrauded them.

About 1868 his "News" gave a review of my "Programme der Atom-Mechanik," of 1867, showing that the writer of that pretended review had never read, probably never seen, the book he abused.

After the publication of my *True Atomic Weights* in 1894, I sent to Mr. William Crookes, on September 4, 1895, a complimentary copy with a letter, stating that the book was sent to him personally (not as editor, *not for review*) and referring to the action above mentioned, supposing that some of his writers had imposed upon him, it seeming incredible to me that a man in his position could have committed so low an act.

That the low act was Mr. William Crookes' own, is fully proved by the ranting and denunciatory *review* given by this same pettifogging individual himself in his Chemical News, vol. 72, pp. 231–232; 1896. This article had entirely escaped my notice, until recently, when I looked up some experimental results of Lord Rayleigh.

This shows that Mr. William Crookes has a very crooked character, that can hardly be excused on the ground of his nauseous spiritualistic record.

After stealing the personal copy for a "review" he lacked, of course, the courtesy of sending me a copy of his manufactured misrepresentations, made deliberately to mislead his subscribers. Such is editor Crookes.

Is he really too ignorant to comprehend ?

It may, therefore, appear strange that I give a position of great honor to this name Crookes, in line with the greatest analytical chemists, from Berzelius to Seubert.

I hereby specially give notice that I have exclusively reference to the *analytical chemist Crookes*, and to his own laboratory record only after freeing the same from the blunders, fraud and folly which seem inseparable from the personality known as William Crookes.

The Atomic Weight of Thallium.

Ad rem: Under these conditions it will be advisable to determine the atomic weight of thallium, independently of the earlier work of Crookes.

There is the electrolysis of thallium sulphate effected by *Lepierre* in 1893. He made 3 determinations on between 1 and 3 grammes of the sulphate. The extreme ratios run from 954 to 945, exhibiting a range of only 9. The mean analytical ratio is 0.80 953, which is only 1 high, for the atomic ratio is

$$Tl_2 : Tl_2 O_4 S = 408 : 504 = 0.80\ 952.$$

The same chemist made 2 determinations on between 2 and 4 grammes of thallous oxide, which he dissolved and submitted to electrolysis.

The atomic ratio for this process is

$$Tl_2 : Tl_2 O_3 = 408 : 456 : 0.89\ 474,$$

while Lepierre obtained in the first determination 13 more, and in the second, 1 more; his mean thus is only 7 high.

Accordingly, there can be no doubt about the atomic weight of thallium. It is 204, exactly.

We have a number of other determinations, but they are mostly made according to faulty methods and therefore do not count here.

Thus, the inferior silver chloride process, according to

$$Tl\,Cl : Ag\,Cl = 239.5 : 143.5 = 1.66\ 937,$$

gave Lamy, 1863, values differing in range by 938; the mean was 23 low. Such determinations are worthless, of course.

Hebberling, in 1865, obtained a mean 472 low, and Wells and Penfield, 1894, obtained as mean 405 high. This shows again simply that the method is bad, and that these chemists did not know it.

The reaction

$$Tl_2 O_4 S : Ba\,O_4 S = 504 : 233 = 2.16\ 309,$$

has also been used, but is notoriously far from reliable.

Lamy, in 1863, obtained a mean 611 high, in 3 determinations.

Hebberling, in 1865, also made 3 determinations; the range was nearly a thousand (923).

All these determinations are simply worthless.

Accordingly, the determinations of Lepierre establish the atomic weight of thallium as 204, independent of any work done by Crookes at any time, before or after the work of Lepierre.

We are now able to take up the work of Crookes for separate and independent consideration.

Crookes' Determinations of Thallium.

Ad hominem: We have no access to the original publication of Crookes in the Philosophical Transactions for 1873, where it begins on page 277, according to Clarke's Constants (1882, p. 95, and 1897, p. 185), the only record at hand giving the weighings and ratios of Crookes.

It may be interesting to the student to read the general statement of Clarke from this record. The italics are ours. The quotation may be read identically the same in *both* editions of Clarke, of 1882, p. 95, and of 1897, p. 185.

"In 1873, Crookes, the discoverer of thallium, published "his final determination of its atomic weight. His method "was to effect the synthesis of thallium nitrate from weighed "quantities of absolutely pure thallium. No precaution "necessary to ensure *purity of materials* was neglected; the "*balances* were constructed especially for the research; the "*weights* were accurately tested and their errors ascertained; "weighings were made partly in air and partly in vacuo, but "all were reduced to *absolute* standards; and unusually large "quantities of thallium were employed in each experi-"ment."

"In short, no effort was spared to attain as nearly as "possible *absolute precision of results. The details* of the "investigation are *too voluminous*, however, to be cited here; "the reader who wishes to become familiar with them must "consult the original memoir. Suffice it to say that *the* "*research is a model which other chemists will do well to copy.*"

So high praise from that quarter we have good reason to deem a positive indication of gross errors hidden by a pretentious show of extraordinary precision. We shall find these indications exact in regard to the work of Crookes. The chemists who have adopted this "model" have reason to regret it. Compare Ramsay and Aston, under Boron, in Part III.

Every material thing, substances, balances, weights, all have been most scrupulously tested and verified, we here are informed through Clarke.

But how about the experimentor himself, Mr. William Crookes? Has he been tested?

Yes, the marvelous concordance proves that *in the laboratory* he was not "crooked," but went the narrow path that leadeth to the goal of minute probable errors. In other words, Mr. William Crookes did agree very closely with Mr. William Crookes in all laboratory work throughout probably many months.

But *Chemistry is* NOT *merely a fine manual handicraft of the laboratory ; it is not merely an Art, but is also a Science ;* in my humble opinion, it is the science of sciences.

Have we any assurance that Mr. William Crookes, the expert manipulator and chemical artisan, was really a *chemist* or that Sir William Crookes to-day has become a real chemist, in knowledge and understanding as well as in weighing, igniting and dissolving?

I shall not pronounce judgment now; but examine his work, which alone must testify hereto.

Testing the Laboratory Work of Mr. Crookes.

The unit of weight used by Mr. Crookes is the grain. The weighings are recorded to the *millionth of the grain*—which is almost down to the hundred millionth of the gramme. There is accuracy, at least on the face of it, in print; we shall come back to this subject.

In this first examination we next note the amount of metal used. It ranges, in the ten determinations, from 12 to 30 grammes, being from 180 to 500 grains in the first determination. Certainly ample thallium was used, possibly too much, in the first determination. We shall examine.

The ratio stated for 100 parts of thallium we convert, by moving the point, to our usual *analytical ratios;* here the amount of thallium nitrate obtained from one unit of metallic thallium.

These analytical ratios are given to 6 places. Droping the last decimal, being visibly worthless, and observing common restrictions, we have our own five place analytical ratios.

The mean of the ten ratios is 1.30 391; extremes are 393 and 388, giving a range of only 5 in the fifth place. That is an extremely minute range. The concordance of the determinations is truly remarkable.

The chemical process, expressed in our Standard Atomic Weights is

$Tl\,O_3\,N : Tl = 266 : 204 = 1.30\ 392.$ Chg. 15 low.

Now it is passing strange that the analytical ratio of a series of ten determinations made in London in 1873, should within a single unit in the fifth decimal agree with our absolute atomic ratio.

Did Mr. William Crookes undertake this work to prove that our "theory" is correct? Hardly, as he but a few years previously had held up to public ridicule our "Programme" of Atom-Mechanics, while showing in his abusive paper that he certainly had not read it. He simply tried to mislead his subscribers and thus to defraud them.

But really, the case is still more astonishing. For upon closer examination of the ratios we notice that only the first is two below 390; we might, therefore, exclude it as the first, not quite successful effort made. We want to treat Mr. William Crookes kindly. But at the same time we must say that the total weight of thallium used in No. 1 is by far the highest. We shall have to come back to this point.

In ordinary determinations, where even fine work is done, we would not mind a variation of two units in the fifth decimal. But here we have something extraordinary indeed in apparent precision. Therefore, let us drop this deviating determination as due to the first trial of a master workman.

There then remain 9 determinations. Now all 9 analytical ratios are identical in the first five digits, 1.3039.

The *fifth* decimal is twice 3, thrice 2, twice 1 and twice 0. The aggregate is 14, mean of nine 1.5.

Hence, the mean analytical ratio of all 10 determinations except the very first, excluded by us for good reasons, is 1.30 3915, which is only half a unit in the fifth place below our atomic ratio calculated from our atomic weights.

This mean analytical ratio must be considered absolutely identical with our atomic ratio.

As the range is also, for these 9 determinations, only 3, and as these slight differences are well distributed, we are indeed face to face with something remarkably excellent in apparent accuracy.

Since now a rise of 0.1 in the atomic weight 204 of thallium produces a lowering of the analytical ratio of 15 units in the fifth place, the half a unit actually low represents a rise of only one thirtieth of such a tenth, that is one three-hundredth of a unit, or only 0.003.

Taking the excess of this analytical ratio as we find it, the atomic weight of thallium corresponding thereto would be 204.003.

We can therefore state, that the 9 determinations of Mr. Crookes of 1873, demonstrate the true atomic weight of thallium to be 204 exactly, and that the possible deviation is not more than three thousandths of a unit.

Now this is most satisfactory to me. Mr. Crookes has done a great work for "my theory" which he has abused, and for our work in general, which Sir William Crookes has editorially denounced like a barbarian and a brute.

Crookes Annihilates Stas.

But Mr. Crookes has done much more than this. He *has furnished us experimental determinations of the highest order, absolutely and totally annihilating the entire System of the Stasian Atomic Weights.*

Now, Sir William Crookes, in that notorious editorial against me, declares himself once again for this system of Stas; coarsely expresses his apparent hatred of me, because I have dared to show this Stasian System of Atomic Weights to be wrong, and here—he himself proves I was right!

Evidently, Sir William Crookes, in 1896, does not understand scientific chemistry any better than did Mr. William Crookes, in 1873, when he failed to understand the scientific signification of his own very good laboratory work.

Just let us see for a moment!

The synthesis of thallium nitrate effected agrees to the thousandth of a unit with our standard atomic weights.

In this formula of the nitrate enters the standard atomic weight for nitrogen as only 14 *exactly*.

But Sir William Crookes and nearly all chemists of the world adopt the Stasian Value $N = 14.04$. They decide it by vote. They denounce me personally for showing Stas to be in error. They would burn me as a chemical heretic, if they could.

Now a very simple calculation with $N = 14.1$, shows that the atomic ratio will be 49 high for this change of only 0.1 in the atomic weight of nitrogen.

As a matter of fact, the mean of the 9 determinations of Mr. William Crookes is only ½ a unit low in the fifth place, that is only one hundredth of the tenth or *one thousandth*, making nitrogen one thousandth low or 13.999.

If Sir William will take pencil in hand and read the preceding paragraph a few times carefully, I hope he will understand that what we therein assert is a very simple statement of fact, resting entirely upon the exquisite laboratory work of Mr. William Crookes, and requiring only the rudiments of mental arithmetic.

But then the value of Stas $N = 14.04$ is forty times as far from the truth as the limit fixed by the laboratory work of Mr. William Crookes, in 1873.

And if the value $N = 14.04$ of Stas is wrong, the value $Ag = 107.92$ falls to the ground; *in fact, the entire mystically—muddy System of Stas' Atomic Weights is totally annihilated by Mr. William Crookes.* For *all* values of Stas are mutually tied together; if any one is proved false, they are all proved false.

This shows plainly, that *Sir William Crookes does not know what he is talking about* when he, in 1896, comes to the rescue of Stas and his School; he forgets that a certain plain Mr. William Crookes published most excellent atomic weight work in 1873, of which, however, the said Mr. William Crookes did not comprehend the meaning, and Sir William Crookes had not yet learned it either—in 1896.

Expurgation of Crookes' Laboratory Record.

Before we can make final and safe use of our above conclusion, namely that the experimental laboratory work of Mr. William Crookes destroys the entire system of Stas' doctrine and all his atomic weights, *we must expurgate the record of Mr. Crookes* in the Philosophical Transactions from the false and invented figures given in Crookes' last three decimals.

We shall draw the line at the thousandth of the grain. That is about half a tenth of a milligramme.

Modern scientific writers delight in pointing out the supposed depravity of priest and priesthood in early Christian and in pagan times. They give picture and word of scientific tricks played upon the faithful in olden days, at Rome, at Athens and at Memphis.

Will not our modern exact scientists, such as for example Sir William Crookes, soon be held guilty of more despicable depravity by this infinitely more criminal scientific trickery of their pretended exact determinations being even less substantial than the incense of the priests of old ?

Is it not a greater fraud to present to the Royal Society a host of numbers, claiming to represent actual data of determinations, made by using our highest instrument of precision in one of its most perfect forms, when the numerals so presented and thereupon published to the world in the big Transactions of that Royal Society, are palpable frauds and inventions to fully one third of the entire set of numbers ?

The old priesthood did preach a mystery, and legitimately used many phenomena, precisely as the parable was used about nineteen centuries ago, and precisely as we use certain illustrations; but modern science first of all is supposed to present the facts of nature by experiment and observation, and when the pretended record of such experiment or observation is falsified in any way, a real sacrilege has been committed—as it has been committed by Mr. William Crookes in the glaring case under consideration.

We declare and shall prove all figures below the thousandth of a grain to be absolute invention and imagination on the part of Mr. William Crookes—intention or not, makes no difference as to the fraud upon the chemical public of the world at all, a fraud that has been upheld with dogged persistence for over a quarter of a century, by just such editors of chemical journals as Mr. Crookes.

The following table gives the *residue of fact* as the weighings, and also the *analytical ratios* as calculated by myself from these values exclusively:

RECORD OF WEIGHINGS
expurgated by Gustavus D. Hinrichs, in 1901,
from the record printed in 1897,
by F. W. Clarke, in his "Constants of Nature,"
which was declared to have been copied from
THE PHILOSOPHICAL TRANSACTIONS FOR 1873 OF THE
ROYAL SOCIETY OF LONDON,
and which was given as a true copy of the
Record of Weighings
declared to have been made by
Mr. William Crookes, of London.

No.	Thallium.	Nitrate.	Analyt. Ratio.
1	497.973	649.295	1.30 388
2	293.194	382.304	393
3	288.563	376.264	393
4	324.964	423.720	390
5	183.790	239.646	391
6	190.843	248.843	392
7	195.544	254.973	392
8	201.816	263.148	390
9	295.684	385.544	391
10	299.203	390.136	392

Grains.

The only result requiring one word of explanation is that of No. 3, where my calculation gave 2.45 in the last place (the fifth). I have properly put this as 3, since the second 5 raises the first 4 to 5 under rule. The record of Crookes, as given by Clarke, gives 2.6, which rounds off to 3, exactly as ours.

Now, how do these, our own calculated analytical ratios compare to those given by Clarke (and therefore by Crookes), pretendedly based upon the *weighing to the millionth of the grain?*

They are absolutely identical with the analytical ratios used by us and taken from Crookes by Clarke.

Hence, firstly, all we have said as to the actual value of these ratios—*and the corresponding true weighings of Crookes to the thousandth of a grain*—remains.

The true atomic weight of nitrogen is 14 and not the Stasio-Crookes' value of 14.04; all Stas' values are false, according to the expurgated Crookes.

They are absolutely identical—these analytical ratios of ours with those of Clarke-Crookes;

Hence, secondly, this constitutes a most absolute demonstration of my declaration, above given, that

> the three last decimals in the weights given by Mr. William Crookes are pure imagination, absolute invention; that these weighings were never made, never had any existence as experimental facts, nor ever were actually used by Crookes, for they did not enter his resulting analytical ratios at all.

How did Mr. William Crookes get the false weights he submitted as experimental data to the Royal Society in 1873?

Of course, not by the use of actual weights, but by *calculating* the weight by the oscillation method.

Now, as we have had to show repeatedly, in all such cases of calculation it *is supposed that the calculator knows enough about what he is doing to stop when he has reached the limit of precision,* and not to go on calculating till he gets tired.

Mr. William Crookes, in 1873, was very much younger than now, and while lacking the necessary scientific knowledge of his actual degree of precision did not easily get tired—but kept on calculating briskly—till he got three places beyond the ken of his balance. Of course he kept eyeing his beloved balance; he looked at the fine machine.

The recorded weighings contain 9 and 10 digits, and require ten—place logarithms for calculating the analytical ratio. Did Mr. Crookes use such tables? Has he ever seen such tables?

If he should claim that he calculated his ratios by hand and not by logarithmic table, why did he stop at paltry six decimals, when 10 place numbers of observation would require 10 place quotients?

In the analytical ratios—the only result of value to us—these three invented or imaginary decimals have entirely disappeared again—without leaving the slightest trace. They evidently were *absolutely spiritualistic decimals and entirely imaginary numerals*, though they were presented to the oldest scientific society of the world as data of observation and experiment—and were so recorded and published—and have been so taken by the Chemical World, lo! these thirty years!

The three last columns of decimals, presented by Mr. Crookes, in 1873, to the Royal Society of London, as actually observed weights of the metal thallium and its nitrate,

were absolutely nothing but imagination, pure and simple, from beginning to end,

constituting an imposition on the Royal Society of London, and

a fraud upon the scientific public of the World.

Accordingly, we find the whole imposition and fraud very properly and very naturally *recommended as a model* by our own scientific fraud at Washington, in his Constants of Nature, both editions: 1882, p. 95, and 1897, p. 185.

And Sir William Crookes, the perpetrator of this scandalous fraud, rants, in 1896, against me about imagination, and what not; let him first apologize to the Royal Society for the gross imposition he perpetrated upon that august body in 1873.

The Royal Society must Act.

There can not be the slightest question about this matter; the demonstration above given is absolutely complete.

We may add, that any one having the time to spare, for a little *exact-science* amusement can take a grab-bag, fill in digits from 0 to 9 in about equal number, say 200 of each, and grab *one* at a time, and record it after the digits given in my expurgation. Shake well after each grab, and keep up till the three fraudulent decimals expurgated by us, have been replaced in number, all drawn from the grab-bag.

These, "*Crookes' Decimals Restored*," will be just as valuable and just as worthless, just as crooked and just as true experimental data, as the original decimals foisted upon the Royal Society as experimental facts obtained by means of the fine balance of Mr. William Crookes, "specially made for that research."

Of course, in grabbing these new crooked decimals, there will be no objection to look at that same or any other fine balance once in a while—it will give a sort of highly scientific air to this performance of crooked "Exact Science."

I hasten to disclaim any personal credit for this method of producing all the grandeur of ten-place data of observation, which has filled our own scientific oracle Clarke with admiration, and made our own Morley imitate the achievement in a work published in grandest quarto style by our own Smithsonian Institution at Washington, in 1895.

Indeed, I believe the keen and truthful observer, Mr. Gulliver, has reported, as an eye-witness, something almost as fine as this grab-bag exact-science work in his noted travels abroad. See his Voyage to Laputa, visit to first room of the Academy, Division of Speculative Science.

An honorable man would hasten to apologize to me for his two editorial abominations; a truly scientific man would acknowledge his error and recall from the Royal Society the invented imaginary data of weighings; but I do not think that Sir William Crookes is built that way nor that he will act that way.

However, the Royal Society, having printed this scientific fraud in its Transactions, will, I think, publish a note of our expurgation to set itself right before the scientific public of the world.

The Foundation of Modern Science.

Modern Science rests on the foundation of experiment and observation; it is supposed to have superseded the so-called *ancient* use of imagination and fancy.

The record of observed facts is therefore something essentially sacred to modern science; and *the Royal Society can not evade its duty because of the high social standing of the culprit, its own member, Sir William Crookes.*

If in these modern days, the persons controlling scientific publications can denounce and persecute honest painstaking investigators who show the error of certain important data used every day in scientific work throughout the world, while those editors and other influential persons themselves have published absolutely fraudulent data of experiment and observation, is not the lot of the independent scientific investigator to-day worse than that of the reformator of the church four centuries ago?

The priesthood when forming a State Church may have abused its high station of power and responsibility as the keeper of the Sacred Truths of Religion; but neither dungeon, torture, nor the stake could prevent the final issue so highly lauded by the liberal scientific public of the present.

SCIENCE IS NO MORE SACRED THAN RELIGION; *when Official Science gets so rotten that its record of fact and observation is rank fraud, the* FINAL ISSUE IS JOINED.

Great ado has been and continues to be made about impositions of relics of Saints; but how utterly innocent such errors appear when contrasted with fraudulent records of observed facts of weighings!

If authority of official position in State Science and in Scientific Journalism is sufficient to prevent just criticism of data of observation and experiment, or of conclusion and principle, the true ground work of modern science has disappeared from view, and the errors and horrors of State

Science will prove themselves as real and as destructive as were the errors and horrors of the State Church in its darkest days.

The *Union of State and Science* (so-called) of to-day is rapidly producing as dangerous consequences to the liberty of conscience and the freedom of research, and even to the very lives and reputations of modern investigators, as the *Union of State and Church* has been condemned for by all "liberal" writers since the burning of Giordano Bruno and John Hus.

The mental—and by necessity the moral—depravity resultant can plainly be seen in Official Science at Washington, by any one who has not closed his eyes.

The Systematic Error of Crookes.

We have above excluded the first determination recorded by Mr. William Crookes. Possibly Sir William may complain about this rejection as a "*criminal selection*" on our part, and insist on his record in its entirety. See his fine editorial of 1896, referred to above.

Being always anxious to accommodate my friends, I cannot allow such a claim as to discrimination or selection take a shadow of footing.

I shall, therefore, now take up this No. 1 of Mr. William Crookes, and shall consider it just as valuable, deserving just as much confidence, as the other nine determinations above specially considered.

But we have an awkward way of looking at the totality of the facts in every case coming under our scientific consideration. We have shown, in our True Atomic Weights, that the amount of matter used is generally a very important factor.

We shall, therefore, now once more consider *all* the ten determinations published by Mr. William Crookes (which determinations we take for granted comprise the *total* number he has made and not a selection) and arrange them in the order of the weight of the metal thallium used. We need, for this purpose, only the round number of grains.

The following table gives the results:

Crookes' Expurgated Results, in the Order of the Weight of Metal Used.

No.	Metal Used.	Analytical Ratio.
5	184 grains.	1.30 391
6	190 "	392
7	195 "	392
8	201 "	390
3	289 "	393
2	293 "	393
9	296 "	391
10	299 "	392
4	325 "	390
1	498 "	388

These results show a small, but *very clearly marked systematic variation of the analytical ratio,* dependent upon the amount of substance used.

Indeed, Crookes has the same failing which we showed Stas to have been afflicted with in his analytical operations; namely, his results vary with the amount operated upon. True Atomic Weights, 1894, pp. 80–85.

We are, indeed, very sorry to detect the symptoms and signs of this Morbus Stasii in the record of Mr. William Crookes; but the readers now see it is actually there, as a fact; and I guess even Sir William Crookes will admit that we have diagnosed this case correctly.

To allow for the slight variations to which even Crookes is entitled, we shall group these results according to the mean weight plainly marked in the individual amounts operated upon. We find the following results:

Nos.	Mean Weight.	Mean Analyt. Ratio.	Excess.
5, 6, 7, 8	192 grains.	1.30 391	1 low.
3, 2	291 "	393	1 high.
9, 10	298 "	391.5	0.5 low.
4	325 "	390	2 low.
1	498 "	388	4 low.

We have added the analytical excess, in our usual manner.

Here is as plain and sharply marked a systematic variation of the analytical ratio, dependent upon the amount of substance used, as could well be desired.

How sad Sir William Crookes will be at the sight of these columns of figures, showing the Morbus Stasii so strikingly in the exquisite laboratory work of Mr. William Crookes of 1873.

The Morbus Stasii.

This disease I have found especially to affect analytical chemists who have no broad knowledge, but are merely fine chemical operators.

They are artisans, not scientists, in the great domain of Chemistry. But they invariably believe they are great scientists, and invariably denounce all who approach science as a sacred possession of the human intellect, on which an error of any kind is a dark blot, and a false statement of fact an unpardonable sin. For all true science is from God, whatever modern evolutionists may say to the contrary.

Another symptom of the Morbus Stasii is the firm belief of the patient in his own absolute accuracy. It is true they never have themselves tested the accuracy of their work, for the simple reason that they do not know how, being merely routine men, common artisans, working in a scientific field, in a chemical laboratory.

The victims of the Morbus Stasii get raving mad when any one dares question their results, and they abuse such persons in the most brutal manner.

As the number of artisans in the world, in every line, is much greater than the number of real artists and masters, these victims of the Morbus Stasii find most sympathetic reception in the societies and academies; such was the experience of Stas, such is the like experience of Crookes.

Effect of Morbus Stasii on Crookes' Work.

But having pointed out the disease, let us study its gravity and its effects on our conclusions.

The column of the analytical excess added in our last table, shows plainly that if Mr. William Crookes, operating exactly as he did, had used about 240 grains of thallium, he would have obtained our atomic ratio exactly.

This would have been the point on the ascending curve of his errors—if Sir William will for a moment allow us the use of ordinary scientific terms.

On the descending curve of errors, he would also have been able to obtain an analytical excess of zero, though with much greater difficulty, as the curve of errors (our trajectory) sinks rather abruptly. About 296 grains would have been the most suitable weight to take for repeated trials, to get a mean excess of zero.

The trajectory of errors drops quite considerably to the last point observed (500 grains); but as we have not the data of reduction to vacuum, we cannot tell whether there was any deep pit or sudden very low temperature troubling Mr. Crookes, at London, as they have notoriously troubled his fellow sufferer Stas, at Brussels. Mr. Crookes has not taken us into confidence, so we cannot ascertain for ourselves.

Now, will the effects of this Morbus Stasii require us to modify our final conclusions?

We see the total range is 5 in the fifth place, and it is systematic, continuous, in a definite curve or trajectory. (See True Atomic Weights, 1894; pp. 149–151).

The rise per 0.1 on N being equivalent to 49—for which we here may take 50—units high in the fifth place, the range 5 corresponds to a tenth of 0.1, that is to 0.01 on $N = 14$.

The total range or uncertainty due to the Morbus Stasii afflicting Mr. William Crookes in 1873, amounts therefore to 0.01 on the atomic weight of nitrogen.

But this range falls to both sides of the truth, namely, from 1 high to 4 low; and this latter very great depression was due to the patient having inadvertently taken an excessive dose of thallium.

We can, therefore, assure Sir William that avoiding such youthful excesses, Mr. William Crookes would have committed no analytical excess greater than 1 low in the fifth place.

Accordingly, kindly excusing these thallic excesses of the youthful spiritualist Crookes, we may say, that *the normal* *excesses he committed, fell between one high and one low*, giving the mean value the greatest possible force, and making it *identical* with our atomic ratio.

This makes the total range on the atomic weight of nitrogen 0.002 above and below.

Our Final Conclusion.

Thus, in conclusion, our diagnosis of the Morbus Stasii of Mr. Wm. Crookes has permitted us to add rather than to detract from the value of his laboratory work.

And now, if Sir William Crookes will permit me to, I will most cheerfully ascribe all his manifestations of injustice and brutality to the Morbus Stasii, from which he has suffered for thirty years.

If cured by this, my somewhat bitter medicine, if completely freed from this really terrible disease, still affecting so many official chemists in high stations, I shall cordially congratulate Sir William Crookes to the most excellent analytical determinations of the nitrate of thallium he made in 1873, as plain William Crookes, by using reasonable amounts of the interesting and important metal he had discovered.

And I do hope that Sir William Crookes, at last, has found out that our science of chemistry is no longer restricted to the laboratory, but reaches up into the highest realms of truth and wisdom—the source of which is God.

Vain imaginations are not science, and have no place in science; but the ideals of truth, wisdom, and of God, are no vain imaginations and no true science will grow where these ideals have been rooted out.

To present false statements of facts is to lie abominably; in the Science of Nature, to falsify in this way, is to commit a sin against the Holy Ghost, which is an unpardonable sin. It is recklessly committed to-day, throughout the scientific world, to the great hindrance of scientific progress.

May this chapter diminish the commission of this crime, "to the greater glory of God."

XII. THE BANEFUL STASIAN ERRORS.

I must be allowed here to add a few words and to give a few figures which ought to convince any chemist of the utter rotteness and darkness of the muddle of Stas.

Here we have—when stripped of the imaginary decimals—a great experimental work, giving most valuable results. *These results are strictly and to the limit of the high precision of the most excellent analyst, Mr. William Crookes, in absolute accordance with our standard atomic weights.*

The analytical ratios determined by Mr. William Crookes in 1873, for thallium to its nitrate, are worthy of a Berzelius endowed with the means at the service of Crookes.

They leave no possibility of a doubt as to the result; the absolute atomic weight of thallium is 204 *exactly;* the limiting degree of precision of actual determination is the thousandth of a unit.

If standing alone, for this one metal only, it would be marvelous. But when we look back and see that the most perfect chemical determinations on the purest materials and by the greatest and most conscientious masters—Berzelius, Svanberg, Dumas, Erdmann, Marchand, Scheerer, Seubert—absolutely agree with this *one single set of values,* our standard atomic weights, which here coincide also with the results of Crookes of 1873—the evidence becomes absolutely overwhelming.

The possibility that such coincidences could be merely a chance, is utterly and absolutely zero.

These coincidences are the positive demonstration of a general fact, a Law of Nature, a Thought of God.

Our standard atomic weights, used as immutable standards in all our calculations, so that we stand on a firm ground, on solid rock—coincide throughout with the true atomic weights determined by the Master Chemists.

The Labyrinth of Stas.

The atomic weights of Stas—of his labyrinthine group: N, Cl, Br, Io, S, Pb, Ag, Na, Ka—*are one and all utterly false.*

The method of calculation* used by him and by his numerous Re-Calculators, makes it impossible that any *one* of these values can possibly be true if *any other one* has been proved to be false.

Surely, Crookes' syntheses of thallium nitrate prove N = 14.04 to be utterly *false*.

Therefore all the values of Stas are false. Not a single one can be true, if any one single atomic weight of Stas has been demonstrated false.

Now, in this case of thallium, good, sufficient determinations have proved (Lepierre) Tl = 204, exactly.

Hence the excellent determinations by Crookes crushingly prove N = 14.04 utterly untenable and totally false.

Thereby fall all the atomic weights of Stas.

To what extent chemical research and commercial analyses have been falsified will soon become apparent.

Here it may suffice to show the error committed in this manner on the atomic weight of thallium.

Crookes himself found the atomic weight so different from 204 or any whole exact number, that he rested his fight against the so-called Prout-Hypothesis on his own excellent determinations.

Mr. Crookes did not know that he had falsified his splendid laboratory work by using the false value of Stas for nitrogen.

As practical chemist, he knows that if he puts a one thousandth part of strychnine into pure water, this water is poisoned.

He was not sufficiently versed in chemical science to realize that putting an error of 0.04 into N = 14, he falsified the result of his own labors; he poisoned scientific truth.

He next poisoned chemical literature, poisoned his own editorial spirit, poisoned the very atmosphere in which other chemists have to live, exposed to his poisonous pen.

He has held chemical science back in the mud and mire of Stas' labyrinth by his persistent poisoning of all data of truth in his journal, and by his constant attempts at the

---

*Compare that first great humbug-calculation, by Strecker, 1846. See Sebelien, pp. 73-75. "The Method of the Least Squares."

scientific life and honor of chemists, like myself, who tried to point out that poison and wished to remove the same.

Taking Clarke's value, from the determinations of Crookes, exclusively, (namely from ratio (4) on page 187, edition of 1897), thallium is 202.595 in Clarke's supposed Hydrogen unit; hence for $O = 16$ exactly:

$$Tl = 204.139.$$

Ostwald (Physik. Chemie, I, p. 113; 1891) gives

$$Tl = 204.146$$

"accurate to the second decimal," hence 204.15.

Both these valuations of Clarke and of Ostwald, rest upon the value of N of Stas; each one having "selected" his own particular value, but it is essentially that of Stas, say $N = 14.04$.

The fraction 0.15 or 0.14 on $Tl = 204$, is taken to condemn the true values, that they *do not agree with our standard values.*

Poisoned water does not agree with good health or even with life; does that prove water a poison ?

Those not hardened in the errors of Stas during a life time, and who have not undermined their own scientific standing with boisterous and continued declamations on the excellency, perfection, absolute accuracy, mathematical exactness of Stas' values and methods, will not understand that we do not close our work "*right here.*" They think we have demonstrated our case completely.

I do not think that my work is done here. It is true, Moses himself thought it sufficient to keep his people forty years in the wilderness, to get rid of those unfit to live in the land of promise, and our Stasian Chemists have been in such a wilderness for about that length of time; but by means of the crooked scientific press, they control, *in these press-darkened times*, it seems forty years has not been enough.

At any rate, *we shall not rest our case at this point*, but proceed to the consideration of the atomic weights of boron and of nitrogen, which in a most remarkable manner constitute *true and most comprehensive test cases*, each in its own way, covering the entire question in all its essential ramifications.

PART THIRD.

The Absolute Atomic Weights of Boron and Nitrogen.

A. EXPERIMENTAL DETERMINATIONS.

I. THE ATOMIC WEIGHT OF BORON. RAMSAY.

The experimental work done by Ramsay and Aston is of so high an order of excellence that we can, upon that series of determinations alone, base the absolute atomic weight of *boron as eleven exactly.*

For this reason the name of Ramsay is placed at the head of this chapter.

It would be unjust to demand of a chemist more than perfectly honest and reliable quantitative determinations, which involve the preparation of pure materials and the management of the actual operations with the utmost attainable skill.

The published record of the determinations of Ramsay and Aston bear abundant testimony to the fact that the laboratory work was done in this manner. Journal Chemical Society, v. 63, pp. 207-217; 1893.

Ramsay has failed himself to obtain the true value of his determination. This is no fault of his, but of his school; the individual cannot be blamed if he proceeds secundum artem. See pp. 35-36.

By our new method we shall obtain, from these experimental determinations of Ramsay, the true absolute atomic weight of boron and even most valuable auxiliary weights, forming cross-ties, giving strength to the entire system here erected on a field cleared of the errors of Stas.

The chemical process selected is the warming, and subsequent distillation of anhydrous borax with methyl alcohol and muriatic acid, leaving dry sodium chloride as residue. See pp. 52–53.

Both the substance and the final product are fixed, accurately weighable solids.

The quantitative chemical reaction is expressed in the following atomic ratio:

2 Na Cl : $Na_2 O_7 Bo_4$ = 117 : 202 = 0.57 921. Chg. 115 low.

Two series of determinations were made. In the first series, flasks of not very resistant glass were used and found to be attacked sufficiently to vitiate the perfect accuracy of the results, giving as mean analytical ratio 0.57 948 which is 27 high.

This result should, therefore, not be used. It has properly been rejected, for cause stated as above, by the experimenters themselves. Of course our own Chief Chemist put this condemned morsel as a tit-bit into his fragrant olla podrida.

For the second series, flasks of very hard glass (combustion tubing) were used and the error, though not absolutely avoided, was reduced to a very minute amount.

Now the sign of this error is perfectly known; it is an increase of the ratio which, therefore, should come out high, if no other constant error affects the process chosen.

On account of the extraordinary importance of this second series of determinations, I will transcribe the weighings from the journal referred to.

I shall, however, omit the last two decimals of the weights given in the journal. These are beyond the range of positive determination, and therefore not experimental data. They have, as a matter of fact, completely disappeared in all reductions made by the authors themselves. I specially call attention to this fact.

Probably it is the recommendation of Clarke, quoted under thallium that induced Ramsay to *copy* the model of

Crookes, abominably bad in this point. This identical recommendation was made in the first edition of Clarke, 1887, p. 95.—We have quoted it, p. 122, lowest line.

Ramsay and Aston's Weighings, in Grammes.

No.	$Na_2 O_7 Bo_4$.	Na Cl.	Analyt. Ratio.	Excess.
22	5.311 81	3.076 12	0.57 911	10 low.
23	4.780 56	2.770 05	943	22 high.
24	4.990 74	2.892 98	968	47 high.
25	4.723 12	2.736 04	928	7 high.
26	3.313 79	1.918 73	900	21 low.
Extremes 968 — 900; range 68.				
			Mean 0.57 930	9 high.

It will be observed that this admirable series of analytical determinations deviates in the sense indicated (high); however, two of the five determinations are low, showing that the errors have actually fallen on both sides of the atomic ratio!

It is this last fact that gives special value to this series, which also is noted for the very small constant error in the direction foreseen by the trifling action on the glass.

It will be very interesting to obtain a more readily comprehensive idea of the precision attained in this work.

As stated, change of boron to 11.1 would lower the ratio 115. Accordingly 11.01 corresponds to 12 low, and 11.001 to 1 low.

Since the individual determinations fall practically evenly on both sides of our standard high and low, bearing in mind the minute constant error *known* to make high, we can say that the true atomic weight of boron is proved not to deviate as much as 0.005 from the standard 11 exactly.

It must therefore be recognized that the five analytical ratios determined in the final series of Ramsay and Aston, being determinations Nos. 22 to 26 inclusive, authorize us to declare that the absolute, true atomic weight of boron is 11 exactly (diamond-carbon being 12 exactly).

The Correlation of Ratio and Atomic Weights.

In the formula, above used, to express the quantitative reaction employed by Ramsay and Aston, we find, in addition to boron and oxygen, also the symbols of sodium and of chlorine.

We will here re-print the reaction referred to:

$$2\,NaCl : Na_2O_7Bo_4 = 117 : 202 = 0.57\,921.$$

This relation is *not* restricted to boron, but implies a necessary condition for *all* the atomic weights represented therein.

We have already, under the head of thallium, practiced the work we shall now explain a little more fully, while using it *in its broadest way*.

We determined the exact atomic weight of nitrogen by the reaction devised for the determination of the atomic weight of thallium. However, in that case, we used Lepierre's determination for the value of Tl and then used the syntheses of Crookes for N.

But it is evident, that we did really not require the work of Lepierre, and still could have *verified* both Tl and N from the determinations of Crookes.

It is this sort of work we want to do now, and for that reason, we better explain the mathematical principle employed.

The chemical equation, re-printed above, requires *all* the chemical symbols to possess the values stated as standard atomic weights.

Hence the numerical values observed, namely, the analytical ratios, will form perfectly binding *tests* or conditions for *any one* of the true atomic weights of the elements contained in that formula.

$$2\,NaCl : Na_2O_7Bo_4 = 117.2 : 202.2 = 0.57\,962.$$

If we suppose Na to rise by the usual one tenth of a unit (0.1) *while all the others remain constant*, we shall obtain the atomic ratio 0.57 962, which is "41 high," as compared to the above given value for $Na = 23$.

$$2\,NaCl : Na_2O_7Bo_4 = 117.2 : 202 = 0.58\,020.$$

In the same way, for $Cl = 35.6$, *while all the others remain* *constant* and equal to the standard values, we shall obtain 0.58 020, which is "99 high."

In fact, we might even apply this same reaction as a test for the value of oxygen. Supposing O = 16.1 we shall obtain

$2\,NaCl : Na_2O_7Bo_4 = 117 : 202.7 = 0.57\,721$

which value is "200 low" as compared to 0.57 921 found for all standard values, including O = 16 exactly.

But if a rise of 0.1 in the atomic weight of oxygen, O = 16, causes a change of the atomic ratio of "200 low," it is evident, that a lowering of each single unit in the fifth place of the atomic ratio will correspond to a rise of only 1/200 of 0.1 in the atomic weight of oxygen, that is an amount of only 0.0005 rising.

In general, it will facilitate the handling of these minute changes, if we will observe that "change high" corresponds to same *sign*, "change low" to opposite signs of departure and ratio.

Accordingly, the reaction practiced by Ramsay and Aston permits every one of the *four* elements to be tested, as to the exact true atomic weight, *provided we assume the other three constant or fixed*, at their standard values for the time being.

The *rates* at which these variations take place, in the fifth decimal of the ratio for an increase of one tenth unit (0.1) of the atomic weight of the *one* element specified is given in the following table, together with its inverse calculated as shown above.

Change of atomic ratio, per 0.1 rise in atomic weight; and

Change in atomic weight, per rise of one unit in fifth place of atomic ratio.

Change of:	Atomic Ratio.	Atomic Weight.
Boron,	115 low.	0.00 087 low.
Sodium,	41 high.	0.00 244 high.
Chlorine,	99 high.	0.00 101 high.
Oxygen,	200 low.	0.00 050 low.

To readily follow the sign (high or low) it suffices to observe, whether a rise of 0.1 causes high or low, i. e. same or opposite change.

Now, as a matter of fact, Ramsay and Aston found the mean analytical ratio 0.57 930, which we found to be only

"9 high,"

in the fifth place, notwithstanding the fact, that the process *necessarily* gave a trifling value high, due to the action upon even the hardest glass that could be obtained, while the actual individual values fell on both sides of the atomic ratio calculated.

If we multiply the above given values per unit in the fifth place by this actually observed value "9 high," we shall obtain, for each element separately, the following possible *change* in its atomic weight and the corresponding value of the latter:

		3 Places.	2 Places.
Bo	0.0078 low.	10.992	10.99
Na	0.0220 high.	23.022	23.02
Cl	0.0091 high.	35.509	35.51
O	0.0045 low.	15.995	15.995

In words, we have thus established, that the mean analytical ratio being "9 high" according to the excellent analytical work of Ramsay and Aston, proves *that three of the four atomic weights being exactly identical with our standard atomic weights, the fourth will be,*

if Boron, at most, 0.01 low;
if Sodium, at most, 0.02 high;
if Chlorine, at most, 0.01 high;
if Oxygen, at most, 0.005 low;

This shows, for the first time, the full force of our demonstration, extended to *all* the elements involved in any one given chemical process fit for atomic weight demonstrations.

I hope that every chemist will readily understand this method of testing, in its broadest sense.

The general principle is easily stated, and I trust will now readily be understood.

In the chemical process here considered, we have the change of anhydrous borax, $Na_2 O_7 Bo_4 = 202$ to salt, $2 Na Cl = 117$, all atomic weights being taken as their *standard values,* namely

$Bo = 11$, $Na = 23$, Cl 35.5, $O = 16$; exactly.

The *standard atomic ratio* of this process accordingly is

117 : 202 = 0.57 921.

Actually performing the operation five times (Nos. 22 to to 26), Ramsay and Aston found their mean analytical ratio "9 high," that is 0.57 930.

If this "9 high" be ascribed as *exclusively due to*

Bo, its atomic weight must be 0.01 low, or 10.99;
Na, " " " " " 0.02 high, or 23.02;
Cl, " " " " " 0.01 high, or 35.51;
O, " " " " " 0.005 low, or 15.995.

In actual practice, we must arrange it so, that *all but one* atomic weight involved are determined beforehand; thus *that last one* will be determined by the chemical reaction used.

To the student of higher mathematics, we need not say that this process is merely the use of partial differentials, considering only *one* of the atomic weights subject to variation, at a time.

I have also referred this subject properly to the general theory of the variation of constants, so important in astronomy. See True Atomic Weights, 1894, p. 158.

We have purposely taken the entire amount of "9 high" given by Ramsay and Aston, without change or reduction of any kind.

But we do know that this value is itself too high, as indicated by the authors themselves, and as proved by the minute action on the glass.

Probably *half* this excess is all that should be taken into account. Thus all departures would be reduced to *half* the values above given.

II. THE ATOMIC WEIGHT OF SODIUM. ASTON.

We have just seen that, if the "high" of the process be ascribed to the sodium exclusively, the atomic weight thereof can only depart at most 0.02 from the standard 23 and be "high" too; i. e. 23.02.

But we know, that the action on the glass accounts for part of this high, so that probably 23.01 would really be the limit, if the determination could have been made in absolutely resistant glass.

Thus we obtain a determination of the atomic weight of sodium, which we shall credit to Mrs. Aston, who assisted Professor Ramsay in that work.

Of course, this determination pre-supposes that the "other three elements involved," namely, Bo, Cl, O, be known as to their exact atomic weights.

By the general principle just stated, we even need not have actually determined these values.

It is, I trust, fully understood, that the *possible limit* of deviation is perfectly fixed for any and all of the elements in any one reaction.

But as a matter of fact, we have a good determination for boron, independent of the work of Ramsay and Aston, namely, in the boron carbide determination by H. Gautier in the laboratory of H. Moissan, which we shall refer to presently.

Consequently, the above determination for Na = 23 becomes of undisputable force.

The Stasian value, Na = 23.05 is entirely incompatible with the "9 high" of the determinations of Ramsay and Aston; it would have required a mean about "45 high," as is readily seen.

Any such "high" is perfectly preposterous in connection with the admirable work of Ramsay and Aston.

Since sodium has been so completely kept inside the Stasian muddle, this determination here given becomes of very high value. Strange to say, it is about the only reliable determination we can find.

Confirmation of Cl = 35.5.

The Stasian value of Cl = 35.45 is utterly inconsistent with the determinations of Ramsay and Aston.

Evidently, the analytical excess should have been "45 low" instead of "9 high" to warrant the Stasian value.

Such an analytical excess is absolutely inconsistent with the work of Ramsay and Aston.

Consequently, this work disproves Stas' value for chlorine, and confirms Turner's value.

The Silver Chloride Process Tested.

It is a most fortunate circumstance that Ramsay and Aston tried to "*check*" their analytical work by determining the sodium chloride in their final series (after weighing) also by precipitation with silver nitrate, and subsequent weighing of the silver chloride resulting.

The atomic ratio for this additional determination is

$$Na_2\,O_7\,Bo_4 : 2\,Ag\,Cl = 202 : 287 = 0.70\,383.$$

Change to 108.1 gives ratio 49 low.

The weighings given (l. c. p. 215) are:

No.	$Na_2\,O_7\,Bo_4$.	Ag Cl.	Analyt. ratio.	Excess.
22	5.311 81	7.525 87	0.70 580	197 high.
23	4.780 56	6.779 42	517	134 high.
24	4.990 74	7.080 43	489	106 high.
25	4.723 12	6.696 02	536	153 high.
26	3.313 79	4.693 13	610	227 high.
		Mean	0.70 546	163 high.

Accordingly, the above stated change for 0.1 would give 107.67 as the atomic weight of silver; because 163 is 3.3 times the 49.

This result conflicts both with the value of Stas 107.93 and with our standard 108.

Now, as it fails to sustain the value of Stas, and as it greatly deviates from our atomic ratios—sustained throughout in all the best analyses by the best chemists—it follows that *the silver chloride process* in the wet way, even in the hands of Ramsay, gave unreliable, false values, and *is unfit for atomic weight determinations.*

We notice that Ramsay (l. c. p. 216, sub. IV) tries to connect this discordance with the action on the glass; but there even then remained a discrepancy which to our eyes is enormous, namely:

Mean from sodium chloride,	10.966
" " silver chloride, corrected,	10.997
Difference,	0.031

which amounts to one-third of one per cent.

The "correction" applied by Ramsay is itself very questionable—the only fact really demonstrated is the failure of the silver chloride process.

The discordance of these results is even too much for Clarke, who says (p. 175), that "*the discordance*" in the ratio Na Cl : Ag Cl "*was noted,* (p. 52) *which again appears here,*" and "*entitles it to comparatively little credence.*"

Now, a bad egg will not hatch, and a bad analytical process cannot give good analytical ratios. We should not use processes known to be bad in determining atomic weights any more than we would set a hen on eggs known to be bad.

Is it really exacting to demand of a recalculator in the government service to use as much intelligence and common sense in the revision of the atomic weights, the fundamental data of chemistry, as a farmer cheerfully expends when setting a hen?

Professor Clarke evidently thinks such a demand is unreasonably exacting; for he puts the five bad eggs of Series I and the five good eggs of Series II under his hen—and gets what he always succeeds in getting: rotten results for his fragrant olla podrida.

This "discordance" appears (on page 215 in the publication of Ramsay cited) most strikingly under the heading of

Boron Atomic Weight, Calculated from:

No.	Na Cl.	Ag Cl.	Differences.	Hinrichs.
22	10.983	11.071	.088	11.008
23	.955	.024	69	10.980
24	.936	.003	67	10.959
25	.968	.039	71	10.994
26	.992	.091	99	11.018
Mean	10.965	11.084	.079	10.992

It is seen that the two sets of atomic weights calculated by the chemists themselves (by the use of Clarke's false Smithsonian Atomic Weights, see pp. 33–34, supra) are irreconciliable; those calculated from the silver chloride determinations being, on the average, 8 hundredths higher than those calculated by means of sodium chloride.

Now "8 hundredths" is very nearly "one per cent" on $Bo = 11$.

A discordance of 1 on 100 is rather tough "Exact Science."

In the last column I have added my own calculation of the atomic weight directly from the analytical ratio *a* found (from Na Cl).

Since, by standard atomic weights, the anhydrous borax is 158 + 4 Bo and sodium chloride is 117, and the analytical ratio is the latter divided by the former, it follows that

$$4\ \mathrm{Bo} = q - 158 \text{ where } q = 117 : a.$$

From this expression our atomic weight given above is calculated.

We notice that all our values are very near 11 and oscillate to *both sides* of this number; the mean is only 8 thousandths less than 11.

The comparison of this last column with the first under Na Cl shows strikingly that the constant error has disappeared by taking our standard values for the auxiliary atomic weights, namely, Na = 23, and Cl = 35.5, instead of the Clarke values Na = 23.05 and Cl = 35.45, used by Ramsay.

This gives an additional demonstration that the commonly used auxiliary values are false, and to what an extent their small errors will affect the final value of good chemical determinations. Compare page 33.

It is, of course, thoroughly understood that we merely wished to show the effect of taking our standard values also in this form of the direct calculation of the atomic weight.

We are, however, only interested in the one true absolute value of the atomic weight of boron, which is eleven exactly, as we have proved.

Determinations by Henry Gautier.

In the chemical laboratory of Professor Moissan, of the University of Paris, Henry Gautier has recently made four series of determinations of the atomic weight of boron.

The results have been presented by Moissan himself to the Academy of Sciences of Paris, and are printed in the Comptes Rendus, T. 129, pp. 595–598 and 678–681 for 1899.

A more complete record of this work is published on pp. 352–382 of Tome 18, of the Annales de Chimie et de Physique, Paris, 1899 (direction includes Moissan).

At the annual public session of the Academy, on December 17, 1900, the Vaillant prize was awarded to Henry Gautier for this investigation on the recommendation of the entire section of chemistry (Troost, A. Gautier, Moissan, Ditte, Lemoine), represented by *Moissan* as "rapporteur." See Comptes Rendus, T. 130, pp. 1110–1111; 1900.

It is stated in this report that the determinations of Ramsay, open to objections, made this new research desirable.

Thereby this section, in recommending the award, assert that the work of H. Gautier is superior to that of Ramsay.

They finally admit 11.01 as the value, and accentuate the special discussion of the "probable errors" by H. Gautier. See p. 34, supra.

If this work by H. Gautier is superior to that of Ramsay, we ought to remove that name from the head of this chapter on boron. Let us examine the work of H. Gautier, produced under Moissan.

Henry Gautier, 1899.

1. SULPHIDE: Change to 11.1.
   $Bo_2 S_3 : Ba O_4 S = 118 : 699 = 0.16\ 881$ 20 high.
   Det. 4, Extr. 897 — 874; 23. Mean 4 high.
2. CARBIDE:
   $Bo_6 C : C O_2 = 78 : 44 = 1.77\ 273$ 2360 high.
   Det. 2, Extr. 293 — 224; 69. Mean 15 low.
3. BROMIDE:
   $Bo Br_3 : 3 Ag Br = 251 : 564 = 0.44\ 504$ 10 high.
   I. Det. 5, Extr. 516 — 505; 11. Mean 8 high.
   II. Det. 4, " 515 — 509; 8. " 9 high.
4. CHLORIDE:
   $Bo Cl_3 : 3 Ag Cl = 117.5 : 430.5 = 0.27\ 294$ 20 high.
   Det. 6, Extr. 292 — 279; 13. Mean 10 low.

Looking at the determinations as equivalent (which they are not) we get, as a first, general (though not exact) view, the following estimate:

No.	Analytical Det.	Analytical Excess.	Final Excess. High.	Final Excess. Low.	Atomic Weight, Mean Excess.
1	4	4 high	16	—	0.020 high.
2	2	15 low	—	30	0.000 low.
3	9	8 high	72	—	0.118 high.
4	6	10 low	—	60	0.050 low.
		Sums	88	90	

Practically equal, high and low, hence *final excess zero,* and Bo equal to standard 11, *exactly.*

To obtain the true excess on the atomic weight 11, we must divide the excess of the mean, by the change per tenth; the quotient will be in units of first decimal place.

The results so obtained are stated in the above table under the head of: atomic weight, mean excess.

A mere glance at this column shows that the chloride and bromide give great deviations, that for the sulphide being smaller; only the carbide gives less than a hundredth.

In this connection we must bear in mind, that the process—the barium sulphate process—used for the sulphide, notoriously is analytically very dull and atomically also; 2 units in the fifth place corresponding to 0.01 on the atomic weight.

The chloride process is atomically as dull, and the substance not directly weighable in the sense of Berzelius.

The bromide process is even twice as dull, atomically, and the bromide equally unweighable.

My Carbide Process.

This leaves only the *Carbide Process* atomically most sensitive of all, and indeed, one of the most sensitive processes in chemistry; but only two determinations were made, and these on very small amounts of substance.

Accordingly there is absolutely nothing in this research of H. Gautier made under Moissan, and so highly honored by the Academy of Sciences at the direct recommendation of the same Moissan, that we must repell the declaration or official imputation that this research is even comparable to that of Ramsay.

Tested by our methods, which were publicly taught in the halls of the University of Paris by Schützenberger under the presidency of Friedel as far back as 1896. (Actualités Chimiques, pp. 4-17; 1896, also his standard work: Chimie Generale, Paris, 1898, pp. 143-152), and repeatedly represented in the Comptes Rendus and in my work "The True Atomic Weights, 1894," so well known at Paris, we find nothing (with one exception) in either the method of work or in the results obtained by H. Gautier comparable in value to the work of Ramsay which this is intended to supplant or to support.

The one exception referred to is the carbide process, which was borrowed from pp. 175–176 of my True Atomic Weights, 1894, in the hands of Moissan, and favorably known throughout the scientific circles at Paris, as I could readily prove by fac-simile letters of prominent members of several sections of the Academy of Sciences of Paris.

Even if Moissan had overlooked this most important part of my book, he and his section of chemistry must have been reminded of it by the very pointed *second paragraph* of my paper in the Comptes Rendus T. 131, pp. 1712–1714; 1900, published six months before the section reported to the academy in December, 1900.

This entire communication of mine "On the True Atomic Weight of Boron" is printed at the close of this chapter, in literal translation from my French original, printed June, 1900, in the Comptes Rendus.

In thus ignoring my work, while making use of my method, the section has not treated me as dishonorably, as it has two of its own leading members whom three of said members have followed to the grave barely a couple of years ago.

La Pleiade de Chimistes d'Alsace.

These two great chemists, Schützenberger and Friedel, together with Würtz and the officially so much persecuted *Gerhardt, constitute that famous "Pleiade of chemists of Alsace" (and, therefore, German in real origin as well as in name) so generally counted in as *French* chemists, for example, by Lemoine in his admirable éloge of Friedel, spoken on July 23, 1900, before the academy.

Why should the public teachings of these great men be treated with contempt by Moissan *and his present* associates?

And if my work were unworthy, if these last chemists of the "Pleiade of chemists of Alsace" had been mistaken in their public teaching at the University of Paris, and in

---

* See his biography, by his son and Grimaux, published in 1900.

the endorsement of my principles, why should Moissan use my method, the carbide method, the only one which is good in the four employed by his student, and say nothing about it?

Does Monsieur Moissan believe that he can commit such an act without the chemical world taking notice thereof?

Monsieur Henri Moissan.

This carbide method is fully set forth in my book of 1894, the page cited in the Comptes Rendus for June 18, 1900; the method is direct, connecting atomic weights to my standard of matter, the diamond, by oxygen as the only link, common to all.

To make matters worse, Moissan let the very excellent young chemist, Henry Gautier waste his time and skill on the crude and dull methods of his own (see p. 47, last 3 paragraphs, supra), while he had him use my method, exact and sharp, only on two determinations and upon very small amounts of the carbide.

The precise chemical method of reaching the combustion, removing by liquid chlorine the boron, is worthy of Moissan, and cheerfully recognized by me; but the method of atomic weight determination used, he has taken from my publication, without giving due credit therefore.

Finally, *Moissan has caused his special laboratory student, Henry Gautier, to falsify his most excellent laboratory work* by reducing the same by means of the false German atomic weights. See p. 34, supra.

I have, in my paper of June 18, 1900, which now follows, and which I supposed sufficiently clear and comprehensible to any chemist, and especially in the next paper, shown up this falsification and therefore spoliation of excellent French laboratory work by false German standards, selected by Moissan.

If the "new era" of French chemistry under Monsieur Henri Moissan shall continue in that way, French chemists will suffer much more than I by the dishonorable and overbearing manner in which Monsieur Moissan has acted and forced his colleagues to cover his action with their name.

His dishonorable treatment of his dead colleagues, the last of the Pleiade of Chemists of Alsace, I leave to his living confreres to deal with.

How utterly ignorant of all chemistry, not merely manual in character, Moissan must be, is palpably evident in the prominence given in his Rapport, accentuating the "probable errors" having been determined and "discussed," when as a matter of fact he thereby shows that he does not even know how that useless humbug (see pp. 11–18) is calculated; see p. 363 of the "Annales" above cited, where the essential coefficient (nearly ⅔) is omitted, so that *all* the "probable errors" calculated and discussed are 50 per cent. too high!

I hasten to conclude "*cette affaire*" by stating in as plain words as I can command, that:

I. The analytical work of Ramsay for the determination of the atomic weight of boron is conceived in the spirit of Berzelius and was carried out in a manner equally high, and that as a matter of fact, it permits us to establish the atomic weight of boron, as we have shown.

II. The three methods proposed by Moissan are either notoriously dull analytically (sulphide) or are contrary to the Berzelian spirit (substance not weighable, use of silver process, etc.) and give enormous departures of from 5 to 12 hundredths on the atomic weight of only 11;

III. My carbide method is good, gives excellent results, but has been neglected in favor of his own miserable methods; and Moissan has insulted the memory of his late colleagues as well as myself by taking my method without giving due credit therefor.

Friedel and Schützenberger.

That I have a perfect right to connect these honored names of the "Last of the Pleiade of Alsace" with my own, will require no word of proof in the circles of *la Haute Académie des Sciences*, which I honor and respect most highly, and from many leading members of which I hold tokens of regard and encouragement; but for the benefit of the general scientific public and especially for the younger chemists, I will add the following few special facts, which to state the circumstances fully entitle me:

Friedel, for several years, habitually addressed me as his friend (ami) and treated me as such; I knew him personally since 1873.

In 1896 he accepted, with cordial thanks, the dedication of my General Chemistry, published early in 1897; see pp. 5–9 of that work. And

Schützenberger, in his published lectures and in his great work on General Chemistry, as above specified, has pronounced himself.

Indeed, one of the professors of the University of Paris, recently wrote me: "Schützenberger avait une admiration véritable pour vos idées."

Of Friedel, the honorable successor to his "fauteuil" in the academy, declared, when taking that chair, (July 23, 1900):

> "*Il avait la foi:* foi dans ses opinions chimiques;
> "foi aussi dans ses opinions philosophiques et dans
> "les devoirs pratiques qu 'elles imposent."*

This man of faith, as well as of science, was greatly depressed after November 15, 1897, and died a little over a year after that date. Those who can read ——— may find the reasons in the Comptes Rendus of that date.

His whole scientific and personal character was rudely shaken by an assault on the atomic theory on the part of a noted chemist who himself has ornamented the pages of the Comptes Rendus with numerous of the imaginary diagrams of Emil Fischer of Berlin. This assault was seconded by another leading chemist.

It is scarcely thirty years since Dumas acted that way; "c'est toujours la même chose"—only the names have changed. O tempora! O mores!

---

*He was a man of firm convictions, of faith; conviction in his chemical opinions; conviction also in his philosophical opinions and in the practical duties which they impose.

On the True Atomic Weight of Boron.*

“The substance which must be taken as standard of matter for the fundamental data of Chemistry, the Atomic Weights, is the Diamond; $C = 12$, exactly. See: Aperçu du Systéme des Poids Atomiques de precision, fondé sur le diamant comme matière étalon (Comptes Rendus, T. 117, pp. 1074–1078; 1893.)

“The crystallized carbides produced by Mr. Moissan have suggested to me an entire series of direct determinations of precision. See: True Atomic Weights, pp. 175–176, Saint Louis; 1894.

“Mr. Henry Gautier has recently made the first two determinations of this kind, in the Laboratory of Mr. Moissan. Comptes Rendus, T. 129, pp. 595–598; 1899.

“In the *first* determination, 268.6 milligrammes of boron “carbide gave him 151.5 mgr. of carbon dioxide; in the “second determination, 326.8 mgr. of carbide gave him “184.4 dioxide.”

“In order to avoid the introduction of errors in the “calculation or by the use of auxiliary data which might be “inexact, it is best to proceed according to my *General “Method for the Calculation of Atomic Weights*, published “in the Comptes Rendus, T. 116, pp. 695–698; 1893.”

“The common atomic weight of boron being 11, the unit of weight of the carbide ($Bo_6 C = 78$) must give 0.56 410 of the dioxide ($CO_2 = 44$).

“According to the *first* determination, the 268.6 of carbide ought to have given 151.52 mgrs. of dioxide, which is 0.02 mgrs. more than the value obtained by Mr. Gautier.

“In the *second* determination, the 268.6 mgrs. of carbide ought to have given 184.34 mgrs. of dioxide, or 0.06 mgrs. less than the amount found by Mr. Gautier.

“Since these minute deviations are entirely below the limit of the weighings, it follows from the determinations of Mr. Henry Gautier that the common atomic weight of

---

* This is a correct and complete translation of my paper published in the Comptes Rendus, T. 131, pp. 1712–1714; 1900.

boron is also its true atomic weight. See Comptes Rendus, T. 116, p. 695; 1893.

"The application of my general method of calculation gives us a true appreciation of the excellency of the laboratory work made by Mr. Henry Gautier; besides, we were right in our confidence in the great value of the crystallized carbides of Mr. Moissan for the direct determination of atomic weight by means of carbon.

"Mr. Gautier has also made several determinations on the sulphide, chloride and bromide of boron. By the method of the means and the use of German values of the auxiliary elements, he obtains as many values for the atomic weight of boron, and finally adopts the number 11.016 (l. c. p. 681).

"By applying our general method to the experimental data of Mr. Gautier, we find the mean 11.004, which indicates that the true value must be 11 exactly.

"In addition, the laboratory work of Mr. Gautier, in this our direct reduction and without any hypothesis, demonstrates that the German data used in the calculations of Mr. Gautier present several errors. We shall return to this subject."

This is my full communication as printed in the Comptes Rendus, of the meeting of the Academy, held on June 18, 1900.

The report of the Section of Chemistry often referred to, was made at the meeting of December 27, 1900, fully half a year later.

This gives the essential data under consideration as they are on record in this official organ of the Academy of Sciences, of Paris, the Comptes Rendus.

III. THE ATOMIC WEIGHT OF NITROGEN. LORD RAYLEIGH.

The name at the head of this chapter is not that of a chemist, in the common acceptance of that term. *The Right Honorable Lord Rayleigh is Professor of Natural Philosophy of the Royal Institution of Great Britain, London.*

But Lord Rayleigh has produced the experimental determinations which enable us to establish the true value of the atomic weight of nitrogen, and to demonstrate beyond the shadow of a doubt that the value of Stas, N = 14.04, is absolutely false.

We may say, the error of Stas and his school led to the discovery of argon, which discovery in turn proved the absolute fallacy of the results proclaimed by Stas and so viciously maintained by his school.

Density Determinations and Atomic Weights.

The density of true gases is proportional to their molecular weight; and hence, for gases of like molecular constitution, this density is directly proportional to their atomic weight.

From the earliest times, density determinations have, therefore, justly been considered most valued auxiliaries in atomic weight determinations.

The density determinations of nitrogen were used as most valued confirmation of the Stas' atomic weight N = 14.05.

In the first edition of the "Constants of Nature," 1882, p. 50, we find the following table of results for O = 15.96 to which we add the values corresponding to the common standard O = 16. We also arrange the results in the order of their magnitude.

Atomic Weight of Nitrogen. (Clarke, 1882).

No.	For Oxygen.	15.96	16.00
5	Ka Nitrate,	13.97 74	14.012
4	Ag "	13.98 40	14.019
6	Na "	13.99 06	14.026
1	Density,	14.02 44	14.059
3	Am Cl^{ide} to Ag N^{ate},	14.03 30	14.068
2	Am Cl^{ide},	14.03 36	14.069
	General Mean,	14.02 91	14.064
	Range, actual,	0.05 62	
	" per unit,	0.00 40	

We see here really three distinct values of the atomic weight of nitrogen; we fail to see how Clarke can notice any "remarkably close agreement" or really any agreement for a fundamental atomic weight which is pretended to have been established with marvelous accuracy.

We see, as a matter of fact, a *low* value from the nitrate, about 14.02; a *high* value from the chlorides, about 14.07; and a *medium* value from the density, 14.06.

The latest most accurate density determination before the discovery of argon was published by Lord Rayleigh (Journal Chemical Society, vol. 64, pp. 514-515; 1893). We give the actual weights *in grammes per liter* and divide by the standard atomic weights 16, 14 and 1 to get the value for the standard ONE:

	Grammes per Liter.	*Per Unit* Atomic Weight.
Oxygen,	1.42 961	0.08 935
Nitrogen (atmosph.),	1.25 749	0.08 982
Hydrogen,	0.08 991	0.08 991

We notice that the atomic weight appears *not* strictly proportional with the density, for the weight *per unit* of atomic weight is least for oxygen and greatest for hydrogen. This will be found to be a sign of chemical impurity, according to late researches.

If we calculate the atomic weight of nitrogen from the observed densities here given (Lord Rayleigh's, of 1893), we obtain:

Oxygen,	taken at 16 exactly.
Nitrogen,	14.07 34
Hydrogen,	1.00 62

Lord Rayleigh's Discovery.

But Lord Rayleigh discovered that nitrogen obtained by strictly chemical means from ordinary chemical compounds, which gas he termed *chemical nitrogen*, invariably gave a decidedly lower density than when he operated upon *atmospheric nitrogen*, that is, nitrogen from the air—all operations understood to be conducted so as to obtain *pure* products, secundum artem.

This permitted only *one* conclusion, namely, that one or both of these forms of *pure nitrogen secundum artem* contain some material not yet known (*impurity*) in the then state of the chemical art.

Investigation, carried on by this master, assisted by the chemist Ramsay, led to the separation of *argon* from the air. Argon is much heavier than nitrogen, but not removed from it by the purifying absorbents in use prior to 1894 in chemical art.

The density determinations of Lord Rayleigh, published in 1897 (Chemical News, v. 76, 315), for atmospheric air as unit, are copied in the next table, to which I have added a column of atomic weights for $O = 16$ exactly. Of course, for air and atmospheric nitrogen this number must only be taken as a density referred to $O = 16$.

	Density.	Atomic Weight.
Air, free from H_2O and CO_2,	1.00 000	14.47 51
Oxygen, O_2,	1.10 535	16.00 00
Nitrogen (atm.) with Argon,	0.97 209	14.07 11
" (chem.) no Argon,	0.96 737	14.00 27
Hydrogen,	0.06 960	1.00 28
Argon,	1.37 752	19.91 97

Jahrbuch d. Chemie, 1898, p. 4, quotes from Proceedings Royal Society, v. 62, pp. 204–209, the following molecular weights, for $O_2 = 32$:

Nitrogen, N_2	28.060
Carbon Oxide, CO,	27.999
Carbon Dioxide, CO_2,	44.268

From these data we learn, that pure nitrogen, free from argon, has an atomic weight of 14.003 only, according to Lord Rayleigh's density determination.

Incidentally we notice that hydrogen is 1.003 only; also that by subtracting the one oxygen from the carbon oxide, we obtain as atomic weight of carbon 11.999.

Now, since this last value is true (within the limit of precision), namely, conform to the results obtained by Dumas and his followers by the combustion of the diamond, the *true atomic weight of nitrogen* can, according to Lord Rayleigh's determinations, not deviate more than 0.003 from our standard value of 14 exactly.

Also, hydrogen only 0.003 from the standard of 1 exactly.

But the values of 14.04 (Stas) or the mean of 1882 in Clarke, namely, 14.06 deviate respectively 40 and 60 thousandths, instead of only 3 thousandths.

Hence the value of *Stas,* namely, $N = 14.04$, is irreconciliable with the most accurate weighings of pure oxygen and pure nitrogen made up to 1897, by Lord Rayleigh.

Accordingly, we maintain that *these experimental determinations of the density of nitrogen, made by Lord Rayleigh in 1897, demonstrate the falsity of Stas' value of 14.04.*

Density Recognized for Atomic Weight Determination.

In the edition of the Smithsonian Atomic Weights of 1882, the density value falls between the values deduced mainly from Stas' determinations by chemical means—and Clarke uses the density determinations ex æquo with the chemical determinations in his mean for nitrogen, as quoted above, p. 160.

Since the density determinations were much more concordant than the vaunted exquisitely and marvelous chemical determinations of Stas, the "*probable error*" of the density determinations was the smallest of all, only 0.004, while the probable errors of the means of the chemical determinations ran from 15 to 22, or were from 4 to 5 times as large.

Accordingly, the chemical determinations, namely by Stas, were from 16 to 25 times (the squares of 4 and 5) *less* reliable, than those obtained from density determinations, according to the valuation of Clarke.

Therefore the *weight* of the density determinations being about twenty times as great as the weight of the chemical determinations in the Clarkian sense (we might properly say, in a truly Pickwickian sense), the general mean adopted by Clarke is nearest the density value, and *Stas' work is really excluded by Clarke!*

On page 47 of the edition of 1897, we read concerning determinations made by the most admirable Marignac and by our own Huntington and by the famed manufacturer of Tanagra Atomic Weights at Harvard University, as contrasted with those of Stas on bromine:

> "In this case again, as in so many others, Stas' work "alone appears *at the end*, the remaining data having "only corroborative value."

The exclusion of the above chemists in favor of Stas, is simply due to the fact that Stas kept his "probable error" down to 6 in the fourth place of Clarke, while the other chemists above named had allowed their "probable errors" to run up from 30 to 70.

But since these "probable errors" are from 5 to 12 times as large as those of Stas, the *weight* or the reliability of the results of Stas, will be from $5 \times 5 = 25$ to $12 \times 12 = 144$ times as high as that of the other chemists in the Pickwickian Sense of Clarke.

But for these other chemists to count for anything in competition with Stas, they would have had to produce from 25 to 150 times as many determinations as were furnished by Stas.

Since they did not, they were cooly "dropped" out "at the end."

Now, why did not Clarke in the case of the atomic weight of Nitrogen above quoted (p. 160) "*drop out Stas at that end?*"

Why did not Clarke Drop Stas at the End?

And why insist on the excellence of Stas, when de facto the atomic weight of nitrogen proclaimed in the Smithsonian Pickwick of 1882, was not based upon Stas—being excluded on account of the four times greater probable error—but upon the *density determination* of what then was supposed to have been pure nitrogen?

And as the density determination was the most reliable, giving the smallest probable error, why was it discarded when found to be *vitiated by a big constant error,* which affected it notwithstanding its deliciously minute probable error?

Simply because the Smithsonian Publications in question are *not* presenting true science resting on observed facts, but the Pickwickian form so much more palatable to the capacity of Official Scientists at Washington.

Now, in the second edition of the Smithsonian Atomic Weights, the density values of 1882, having proved to be greatly in error, and the new and correct density determinations by Lord Rayleigh being absolutely irreconciliable with the chemical determination of Stas, does Clarke recognize the experimental fact and discard the experimentally disproved and condemned atomic weights of nitrogen, resulting from the chemical determinations made by Stas?

Nothing of the sort; he simply absolutely disregards the experimental work of Lord Rayleigh, because he has long ago become a blind believer in the extreme accuracy and perfection of the chemical determinations of Stas, to such an extent, that he does evidently not even know that the determinations of Stas are themselves mutually irreconcilable! Only under boron (p. 175) he gives a slight indication of discomfort about certain "*discordances.*" See p. 150, supra.

He merely says, that on account of the presence of argon, the former determinations of the density (and hence the atomic weight) of nitrogen, were "all too high, and "unavailable for any discussion of atomic weights," see l. c., p. 60.

This sentence is rather mixed up, and has only a Pickwickian sense as it stands in the book. It really, de facto, implies that the work of Lord Rayleigh is "unavailable."

Clarke adds a few lines further on:

" *Perhaps, at some future time*, when the density of argon " is accurately known and its amount in the atmosphere has " been precisely determined, *these figures may be so corrected* " as to be useful for atomic weight calculations."

"These figures" are the older ones, including those of Lord Rayleigh of 1893. But the conflict established, the error should have been conceded. Lord Rayleigh *has corrected* the older values, as shown, by removing the argon.

Having in my True Atomic Weights of 1894, with great care and at very considerable length, shown that the determinations of Stas are conflicting and false in themselves, not even complying with the *first* and fundamental condition of all analytical work, to give ratios (or percentages) independent of the amount of matter operated upon, I shall not devote any great space in this volume to the fossil chemical errors of Stas.

We shall rather show the utter falsity of the pretended high concordance of the determinations of Stas in one of the subsequent chapters, after having shown once again, that they really do not even approach to a constant value, but vary greatly with the amount of substance operated upon.

Density Determinations by Leduc.

Apparently very accurate density determinations have been made by Leduc of Paris, both for nitrogen and for oxygen, which fully corroborate the results obtained earlier by Lord Rayleigh.

But this apparently excellent laboratory work by Leduc is mixed up with such absurdities and such a lack of knowledge of the simplest general principles of chemical science, that I confess to a doubt about the value even of the experimental work.

The Comptes Rendus (Tome 123, p. 807; 1896), give as the results of the actual weighings of Leduc the following in grammes per liter. We add the quotient obtained by ourselves *per unit of standard atomic weight*, in dividing the oxygen value by 16, the nitrogen value by 14.

	Grammes per Liter.	Per Standard Unit of At. Weight.
Oxygen,	1.42 93	0.08 933 1
Nitrogen,	1.25 07	0.08 933 5

To the fifth place, these quotients are identical.

This proves that the densities—and hence also the atomic weights—of nitrogen and oxygen are exactly commensurable, being in the exact proportion of the numbers 14 to 16, within the errors of determination (to five places full).

Taking the density of oxygen at exactly 16, these weights give the density of nitrogen 14.0007.

Hence for O = 16 exactly, these weighings give

$$N = 14.0007.$$

The possible departure of the atomic weight of nitrogen is accordingly less than 0.001 from our standard value of 14 exactly.

We have used these determinations of Leduc in our General Chemistry, 1897, p. 378.

So much for the experimental work of Leduc.

Now let us look at the other side, to ascertain whether Leduc is scientifically reliable.

Leduc's Atomic Weights.

This is the richest thing in atomic weights which I have found yet; it was presented to the Academy of Sciences of Paris by Professor Lippmann, on August 2, 1897. See Comptes Rendus, T. 123, pp. 299–301; 1897.

The beginning and closing of this communication are too characteristic of the school and routine, and the utter one sidedness peculiar to men of modern science in some of the highest positions.

The opening words of Leduc (l. c., p. 299) are:

"Taking as basis O = 16, *I have admitted* for the atomic " weight of carbon, C = 12.004, which seems to be within " $\frac{1}{10000}$, according to the experiments of Mr. *Van der Plaat* " (Synthesis of $C O_2$)."

By the way, this V. d. P. never made an atomic weight determination of true carbon. See p. 105, supra.

The last paragraph of this same article (l. c., p. 301), is:

"As to sulphur, the experiments of *Stass* (synthesis of the " sulphide) give the atomic weight 32.056. *I shall adopt* " *this number*, although the experiments of Dumas lead to " 31.986 and those of Erdmann and Marchand to 32.005."

Leduc constantly deprives the Hollander of the terminal s in his name, and as constantly attaches it to that of Stas, so as to make it *Stass*—a very unpleasantly suggestive one to English readers. Stas was not that kind of an animal, any way.

By his density determinations, Leduc is compelled to take N = 14.005.

In this condition Leduc says (l. c., p. 300):

“But, according to Stass, we ought to have N = 14.044
“ together with Cl = 35.457 and Ag = 107.929.”

“ The difference of our two numbers is enormous: $\frac{1}{350}$!
“ I have, however, succeeded to explain this, *without putting*
“ *in doubt the excellency universally recognized of the experi-*
“ *ments of Stass.*”

This “success” is the raising of the atomic weight of chlorine by 0.013 and the lowering of that of silver as much. Of course, that will not affect the weight of silver chloride.

What a havoc this little change would play all round the mystic circle or Stas' values, Leduc has not the remotest idea of. He is as innocent of this entire subject as a new born babe. That makes his reconciliation of Stas with truth so funny.

As stated, we suppose that in the physical laboratory of Prof. Lippmann measuring and weighing of gases is done accurately—although the understanding of the rudiments of general chemistry is palpably lacking.

At all events, the weighings of Lord Rayleigh do not need any confirmation, so far as we are concerned; therefore, I was much in doubt whether I should introduce the weighings of Leduc at all.

I beg again to state that it is merely his weight of a liter of pure oxygen and pure nitrogen for which we here introduce Leduc as a witness.

But an experimentor who takes data so readily, though he states they are not true, makes a pitiful witness even as to his statements of weight and measure.

What causes or compels working scientists in some of the great laboratories of Paris to make such exhibitions of either lack of general scientific training or servile obeisance to authority?

The above examples are really the most disgraceful exhibitions of that kind I have ever seen.

In the words of Hamlet, “there is something rotten in the State of Denmark.”

B. THE FOLLY AND FRAUD OF STAS AND HIS SCHOOL.

Determination by Chemical Means.

The atomic weight of nitrogen, established upon the density determinations of Lord Rayleigh, is 14 exactly.

This result is confirmed by the later weighings of Leduc.

Hence the true atomic weight of nitrogen is 14 exactly.

But these determinations are not strictly chemical ones. The Stasian will say so and demand such.

We have already given a most exquisite chemical determination of the atomic weight of nitrogen, according to which it is 14 exactly; the actual precision being as high as 0.001.

We refer to the Synthesis of thallium nitrate by Crookes, having now the perfectly satisfactory determinations by Lepierre of $Tl = 204$.

The question of the true atomic weight of nitrogen is therefore *settled*, both by the physical (Lord Rayleigh) and by the chemical (Crookes) determinations.

But here is the dominant School of Stas. It has put off my True Atomic Weights of 1894, with extrapolation (p. 71), foolish impudence (p. 56), and kindred tricks and bluffs.

I shall, therefore, in this final exposition, neither presume on their honor nor on their scientific intelligence. I shall simply handle the facts in the interest of scientific truth.

I shall wield the facts just as they are, and treat the Stasians exactly as they have shown they must be treated.

My object is *not* to convince them—for I am sorry to say, that most of them really do not seem to have any convictions, either scientific or moral.

Why was the statement, by Lemoine, in the Academy of Sciences of Paris, so remarkable? (See p. 157.)

Would Lemoine have accentuated the character of Friedel in the manner he did, if such a character were the rule and not the exception among the scientific men in some regions?

The Stasians, having corrupted science, have also corrupted themselves. I have the facts at hand. I forbear.

But if my object is not the conversion of the Stasian Chemists, what do I aim at?

I direct my words to the thousands of young men now studying chemistry in Universities and Colleges throughout the world, or having left these institutions within ten years.

These are *the Chemists of the Future*, and it is above all to these Chemists of the Future that I direct my words.

At the same time, the question discussed is so broad in its general scientific character, that I have tried to express it so as to be understood by all scientific students and the scientific public generally.

I shall treat this subject in three chapters.

First, *the Challenge of Stas* will be considered, as made and as answered.

Second, *the Synthesis of Silver Nitrate* will be thoroughly examined into, and the *absolute* lack of concordance of its results will be shown.

Third, the reaction between Silver Nitrate and Potassium Chloride will be critically examined.

This will really end the scientific discussion. But having been compelled to waste so much time on so miserable a subject, we are all, readers as well as writer, entitled to a slight gratification.

We shall present this gratification in two serio-comic historic papers, namely:

> ON THE DISCOVERY OF THE CHANGE OF THE WEIGHT OF MATTER by chemical combination or decomposition; and lastly,
>
> HERESY IN THE CHURCH OF STAS.

With these two historic essays showing the utter rotteness of Stasism, we shall close this discussion, and proceed to the *summing up* of the case.

I. THE CHALLENGE OF STAS.

The exact atomic weight of nitrogen has been known for forty years, we are constantly told; it has been determined by chemical means with a wonderful degree of precision and certainty by *Jean-Servais Stas.* See my True Atomic Weights, 1894; p. 33.

It is 14.041 in 1860; 14.044 in 1865; 14.055 in 1882—according to Stas and his Dutch Re-Calculator, Van der Plaats (True Atomic Weights, p. 34).

It is 14.041 according to his Great Official American Re-Calculator Clarke (edition 1897, p. 71); the probable error being 0.0021 only.

As no one to-day can know chemistry without being able to read German (until 1870 the language of chemistry was French, for till then chemistry was declared to be a French science*), I shall quote the greatest German authority on this Atomic Weight in the learned German:

"Das endgültige Atomgewicht des Stickstoffs ist nach den oben berechneten Untersuchungen von Stas gleich

$$N = 14.0410 \pm 0.0037."$$

See Ostwald, Physikalische Chemie. *Bd. I, p. 110;* 1891.

How thoroughly Professor Ostwald is competent for atomic weight calculations and how fully he understands the proper use of the method of the least squares in the calculation of the probable error of a mean atomic weight, I have tried to show pp. 42–46 of my True Atomic Weights, 1894.

Take these leading authorities—differing more than they ought to, if the work of Stas were what it is proclaimed to be—we must accept N = 14.04 as the value *agreed upon* by these authorities.

Stas himself is on record as to the *degree of certainty* of this value. He has put his statement in a most formidable mathematical form. He—evidently by some mathematical friend, probably A. Quetelet, who knew as little of chem-

---

*Now, under the leadership of Moissan, young French Chemists in the great National Chemical Laboratories at Paris are directed to take their fundamental chemical data from the "Three German Chemists" adopted by a vote of the German Chemical Society—and the said young French Chemists thereby actually spoil and falsify their excellent laboratory work.

This is the "New Era of Chemistry" in Paris. See p. 155, also p. 34.

istry as Stas knew of mathematics—has quoted the precise transcendental formula giving the "*probability y*" as a function of the "*error x*."

Not wishing to shine with mathematical formulæ, I shall not copy it here; the formula is printed in the entire quotation from Stas on this subject, p. 33 of my True Atomic Weights.

Stas gives (also by the aid of that mathematical friend, for Stas himself could neither do that, nor did he ever learn to understand the result he states), the probability calculated from that formula for the different values 14.040, 14.030, 14.020, 14.010 and finally 14.000.

He declares that the value 14.040 (instead of his 14.044) is *possible*, its chances being 3 in 10; that is, its probability was found to be 0.31278. Of these five decimals, four are transparent moonshine. See p. 19.

The probability of N being 14.000 is stated to be

$$y = 0.0 \; . \; . \; . \; . \; 0 \text{ (370 ciphers) } 879$$

and is accordingly declared to be *entirely impossible.*

He berates chemists for using 14, which he has proved to be in error to the extent of * $\frac{1}{350}$.

See complete translation on p. 33 of my True Atomic Weights of the famous passage of Stas in Aronstein's Translation, pp. 322–323, and the original reprinted in the final *Oeuvres* of Stas, vol. I, p. 731; 1894.

Relying on this mathematical result, Stas finally added to his paper here considered, first published by the Belgian Academy of Sciences in the 35th Volume of their Memoirs, issued in 1865, the famous *Challenge to the Chemists of the World*, present and to come. See Oeuvres I, p. 749; also Aronstein's Translation, p. 347; (published Leipzig, 1867)

This challenge Stas repeated in his last work on Atomic Weights, issued from 1876 to 1881. See Oeuvres, I, p. 814; 1894. See also, True Atomic Weights, p. 34.

The gist of this challenge is the request *to repeat his synthesis of silver nitrate*, which he, therefore, considered the most conclusive of all his determinations of the atomic weight of nitrogen.

---

* Leduc quite recently made this identical discovery, see p. 168.

Neither his challenge of 1865, nor its "Renouvelation" in 1881, has ever been taken up. At least, no chemist has taken the trouble to repeat this work of Stas.

I venture to say that no chemist ever will repeat it.

The challenge is *not* a demand to disprove the value $N = 14.04$ claimed to be true; for that value has been demonstrated to be false, at least by myself in my True Atomic Weights of 1894. Some of the most eminent chemists of the time have admitted this, my demonstration, to be final. Here I may only mention the work of Schützenberger in the Actualités of 1896 already referred to, and in his posthumous *General Chemistry*, Paris, 1898, pp. 143–152.

That the method of procedure of Stas cannot give true values has also been pointed out by Schützenberger on the ground of his own experiments; see close of his paper, Actualités, Paris, 1896, p. 16.

We shall here once more *prove it*, *this time by Stas himself*, that his method of determining the atomic weight of nitrogen by his so-called syntheses of silver nitrate is a most remarkable mixture of chemical folly and error, or rather fraud.

Let us take the weighings of Stas as published by himself, and as so frequently republished by his numerous Re-Calculators (see Footnote, p. 75), all fully represented in my "True Atomic Weights," 1894, from page 40 to 69.

Let us plot the results of Stas for *the atomic weight of nitrogen as ordinates to the amount of silver used* as *abscissæ*.

We get in this way, *two curves*, one for the *dried* silver nitrate, the other for the *fused* silver nitrate, and accordingly two entire SERIES of different atomic weights, really an infinite number of atomic weights of nitrogen, in two great sets, the larger for the *dried*, and the smaller for the *fused* silver nitrate. See Plate III, facing page 161 of my True Atomic Weights, 1894, constructed from data printed on page 164.

On account of the great importance of this subject, I will here give a brief summary of the essential data of Stas, and present a new drawing carried to 600 grammes and free from the other curves relating to other work of Stas—also drawn on a smaller scale for the silver used. See Plate II.

We shall, once more, give *all* the essential data, exactly as given by Stas; gross actual errors of Stas we shall point out, as we did in our True Atomic Weights of 1894. But we shall be as *brief* as possible—referring for additional details to the above work of ours, published in 1894.

Syntheses of Silver Nitrate by Stas.

Stas made ten syntheses of silver nitrate from pure silver; we designate them Nos. 1 to 10, in the order in which Stas actually made them. The results were published by him in his *Recherches* of 1860 (Nos. 1 to 8), and in his *Nouvelles Recherches* of 1865 (our Nos. 9 and 10).

The full and complete record is reprinted in his Oeuvres, T. I., 1894; for the first series, Nos. 1 to 8, on pages 342 to 346; for his second series, Nos. 9, 10, on pages 717 to 725.

See also my True Atomic Weights, pp. 75 to 88; 1894; also Clarke, Constants, 1897, p. 63.

Our re-calculation of the analytical ratios gives essentially the same results as all others have found, and as given by Stas himself; the analytical ratio is the weight of the nitrate divided by that of the metal.

In other words, the *analytical ratio expresses the amount of silver nitrate per unit of weight of silver used.*

It cannot be expected that we should again reprint these ratios here; they have been printed often enough, even by ourselves (True Atomic Weights, p. 77, p. 81) and represented graphically on plate I facing this last page just referred to.

As to these data, we must make the following brief annotations:

No. 7 is excluded by Stas himself; hence we cannot make use of it here. He claims to have lost some material. We cannot go back of his statement of fact. We accept it as a matter of course.

From all determinations, Nos. 1 to 5 and 7, we have found that the reduction to vacuum amounted to 21 milligrammes per hectogramme of silver used in the first series. True Atomic Weights, pp. 82–83.

The readers of any mathematical sense will understand and appreciate this very easy and compact way of procedure, as it were en-bloc; and they will also be astonished that this method has not been used by our modern chemists.

How crude the methods in use actually are, passes belief; we may, for example, point to the delightful formulæ used at the great Laboratory of the University of Pennsylvania, see page 16 of the Thesis for Ph. D. by Willet Lepley Hardin, 1896.

If no blunder is made in the use of such a set of formulæ, it surely is no fault of the said formulæ.

For No. 8, Stas allows 30 mgr. only, while this rule would give 42 mgr. Hence *the values of Stas for No. 8, are placed too low.* We have not changed them, but must insist that this error be kept in mind, when the final curves shall be considered in detail. *The error amounts to 40 per cent on the reduction to vacuum.*

There can be no question about this very grave error in the work of Stas. We dare not overlook it. To *correct for buoyancy in this way is to falsify the record of experiment.* It is fraud.

To commit an error of forty per cent on so simple a calculation as the reduction to vacuum, is in effect just as bad as an actual falsification of the experimental data of Stas.

His famous re-calculators, from Leipzig to Washington, have failed to see this palpable error; their spectacles were of so deep a Stasian haze that they may be excused.

But we must remember that the *values No. 8 are recorded too low* by this error of Stas.

We shall simply point it out here, because for 200 grammes (No. 8), we have determinations Nos. 2, 3, 4, 5, sufficiently nearby, corresponding to about 150 and 300 grammes of silver used.

But the case is very different for Synthesis No. 6, this being the most important, involving the highest amount of silver used, namely, 400 grammes; it is therefore the last point that can be actually determined by experiment, in our curves.

For 405 grammes, our rule—deduced from Stas' own work, Nos. 1 to 5 and 7, gives only 84 milligrammes as the reduction to vacuum.

The actual values used by Stas, for both the dried and the fused nitrate, are 150 milligrammes. *Here the error of Stas in calculation amounts to 180 per cent.*

To obtain so great a reduction for the buoyancy of the air, the barometric pressure must have been 54 inches for ordinary temperatures, or the temperature 200 degrees below zero for ordinary barometric pressure.

I suppose that even the blind admirers of Stas do not know of any cave or pit in the Laboratory of Stas four thousand meters deep—a sort of an inverted Mount Blanc; nor will they pretend that Stas could have weighed his wondrous synthetic silver nitrate at a temperature uncomfortably near the absolute zero.

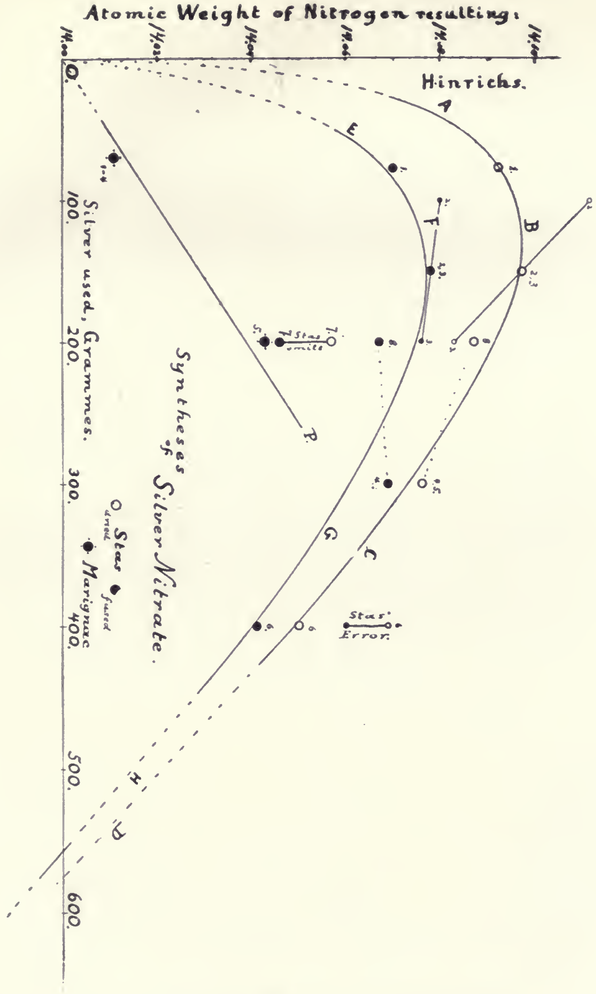
There is nothing to do but to admit the error of calculation committed by Stas. It is palpable.

Such errors were *never* committed by Berzelius—for he properly looked upon this whole thing as a folly, as a straining at gnats while swallowing camels. That even the greatest analyst of modern times, as which we are demanded to consider Stas, in one case out of eight, puffed up the gnat to a good sized calf of a camel (No. 8), and in the other made quite a full grown camel out of it (No. 6), *committing an error of 66 milligrammes* in the weight recorded and reprinted by his admiring re-calculators, is quite astonishing.

This error, at the end of our curves, we cannot permit to remain. We shall mark on our diagrams the points (dry and fused) for No. 6 exactly, as given by Stas and his great and careful re-calculators; but we shall mark them by the words "*false*" or "*error*" and add the correct points obtained by the correct values of reduction, which points we shall mark *true* in this and related diagrams. See Plates I, II, III.

From the analytical excess observed, we calculate in our usual way the atomic weight, here of nitrogen.

PLATE II.



STAS: ATOMIC WEIGHT OF NITROGEN.

If Stas' work were true, this plate would show one single straight line, parallel to the longest dimension of the cut. See pp. 174–182.

These calculated values therefore are based upon O = 16 and Ag = 108, our standard values.

Thus the *absolute* values for N may not suit the Stasians; but I do hope they will understand, that for *any* value of silver, near 108, any changes in the resulting calculated value of N will *vary* in the same way, independent of the precise value of the atomic weight of silver taken.

Besides, I already here promise them to use the most Stasian of all data "next time," namely those of Frank Wigglesworth Clarke.

I am afraid, that the Stasians, after reading both papers, will hardly know which values they like the best, those obtained by our own standard, or those calculated by means of the Stasian auxiliary values for O and Ag, furnished by Clarke.

At any rate, 108 is the *true* atomic weight of silver as we shall prove, and 107.92 is false, as are all the values of Stas. But I here merely wish to call attention to the fact that the difference of 0.08 on 108 can have no influence whatever on the *enormous systematic variations we shall find in the atomic weight of nitrogen*, resulting from the syntheses of silver nitrate made by Stas.

The following table (copied from our True Atomic Weights, p. 164) gives these results:

Stas Syntheses of Silver Nitrate.

No.		Silver, Grammes.	Atomic Weight of Nitrogen. Dried.	Fused.
1		77	14.092	14.070
2, 3		150	14.097	14.078
8		200	14.087	14.067
4, 5		300	14.076	14.069
6	True	405	14.050	14.041
6	False	405	14.069	14.060

We have here combined the neighboring 2, 3 and 4, 5; but on our diagram they are given separately.

These values have been carefully entered on large scale diagram, from which we have had a photographic reduction made, see Plate II. On this diagram we have also inserted the values of Marignac (fused nitrate), forming approximately a straight line running from the origin O to P. We shall not here enter upon details, as we wish exclusively to consider the work of Stas.

In examining this very instructive diagram, in which the ordinates are the atomic weight (excess above 14) of nitrogen, and the abscissæ are the amount of silver used (in grammes) and remembering that all determinations are marked by the proper number, we notice:

I. The determination No. 7 rejected by Stas is far below the value for 200 grammes; hence his reason for rejecting it must have been a good one.

II. The "dried" nitrate cannot have been of quite uniform character. We see "No. 2 dried" much higher above fused, than No. 3; this error of Stas we have practically eliminated by taking the mean of these two determinations, which is marked 2, 3 on the diagram.

The difference in No. 3 is seen to be only one-fourth of that in No. 2, between the dried and the fused nitrate. This constitutes an error of 300% for No. 2 as compared to No. 3.

III. Another such greatly differing "drying" we see in Nos. 8 and the two determinations 4, 5. These last two were both made with 300 grammes of silver, and the results were almost identical.

In this point, 4, 5, we notice the difference between "dried" and "fused" less than half what it is for No. 8. An error of 100% for No. 8, as compared to Nos. 4, 5.

IV. We furthermore notice that this No. 8 lies comparatively *low;* but we expected that, because we have above shown, that Stas committed the grave error of adding 30 milligrammes only, instead of the true value of 42 milligrammes for reduction to vacuum—*an error of 12 milligrammes.*

If I had "corrected" also this error of Stas, the true value No. 8 fused would have fallen almost exactly on the full drawn curve.

V. *The great error of Stas in No. 6 we have shown to amount to 66 milligrammes, too high,* in this case. The points determined by the data of Stas *in error* lie away above our curves. Our true values lie exactly on the curves. We have supposed that Stas, as a matter of fact, was not working in a pit 4,000 yards deep, nor in a room the temperature of which reaches within 70 degrees of the absolute zero; but that temperature and pressure at No. 6 were about the average of what they were for Nos. 1, 2, 3, 4, 5 and 7.

VI. Of course, we would not say that Stas *intentionally* falsified his record for No. 6 and No. 8; but all the same it is a remarkable fact, that by getting No. 6 "high" and No. 8 "low" his values got very much nearer into line than they are in fact; that is, by the curious "*mistake*" committed in the reduction to vacuum, giving in No. 8 a value 40% low and in No. 6 a value 180% high, the false results given by Stas approach more nearly a common mean, than they do without those two "mistakes."

That none of the eminent and *honest* Re-Calculators detected these mistakes of Stas is perfectly natural. "Blind followers of a blind guide" are not expected to see much.

VII. Taking now the points—both for "dried" and "fused" nitrate, marked 1 — mean 2, 3 — 8, raised — mean 4, 5 — true 6 — we can draw a curve through each of these two sets, namely (always see Plate II):

Curve A B C D for *dried* silver nitrate; and
Curve E F G H for *fused* silver nitrate.

VIII. The total *range* of actually determined values runs from 6, fused, true, 14.041 or 14.04, to 2, 3, dried, 14.097 or 14.10, showing a *total range* of 0.06.

IX. But the *range* actually observed is still greater, because No. 2, dried, is 14.11, making the entire actual range 0.07.

X. Since now the pretended value of Stas is 14.04, the range of his own determinations (say only 0.06) is *greater* by 50% at least, than the value he has pretended to determine, namely, the exact excess 0.04 above 14.

XI. When the determinations are of such varying kind, science holds that no determination has been affected.

XII. It is also generally known, and should certainly be known by all who pretend to be experts in this matter of the reductions of experimental determinations, that

When the observed values for a constant follow a definite curve, there are systematic errors present, and the arithmetical mean of the observed values has no sense at all.

The constant here in question is the atomic weight of nitrogen—or the analytical ratio of Stas.

If we accept the famous syntheses of silver nitrate by Stas, to be exactly true as reported (and corrected for palpable errors in No. 8 and No. 6, as shown), what do his actual results prove?

XIII. That the atomic weight of nitrogen is a function of the amount of silver used in its determination; and

XIV. That the atomic weight of nitrogen is higher in dried nitrate than in fused nitrate of silver.

XV. *This difference* between what we may briefly call "*dried*" and "*fused*" atoms of nitrogen is greatest for about 150 grammes of silver taken by Stas for a determination; and that

XVI. This difference, for 150 grammes of silver, amounts to about 0.02, which is fully *one-half* of the entire excess 0.04 claimed by Stas for N above 14 exactly.

XVII. It appears also very plainly, that if *Stas* had continued to work exactly in the manner as he did (but had kept out of pits 4000 meters deep, and stayed away from his laboratory when it got 200 degress below the freezing point) he would have found values for N getting less and less, as he used more and more silver; he would also have found the fused and the dried silver nitrate to differ less and less in weight. See our curves, Plate II.

XVIII. And finally, if he had made a few determinations with 550 to 580 grammes of silver, he would have obtained practically the same weight for his dried and fused silver nitrate, and

XIX. The atomic weight of nitrogen would have been found exactly

$$N = 14.000.$$

XX. Since now the otherwise exacting Stasians have not yet decided which atoms of nitrogen are the true ones, those merely "*dried*" or the lighter "*fused*" ones;*

And since it always has been our desire to help the Stasians out of their holes (even if four thousand meters deep); we shall suggest, that

they use 560 grammes of silver,

follow exactly the method of their master Stas; and will then find N = 14 exactly.

And as then there is no further difference of opinion possible, we shall beg their kind permission to close this little chapter on the challenge of Stas to the chemists of the world.

Postscriptum. I am sure ordinary wide-awake readers, such as our common American students, who have not worn out the seats of too great a number of pants at school, will have taken note of the delectably minute "*probable error*" of the Stasian value for nitrogen, and found a great deal of innocent amusement in comparing this minute probable error with the colossal range of the values found; but I fear my Stasian readers have overlooked this—and hence the necessity for this postscript.

The probable error, according to Clarke is 21, according to Ostwald is 37 units in the fourth place; the mean is 29, that is, 0.0029, for which I think we may be pardoned to put 0.003 or 3 thousandths.

The actual range we found 0.07, which is fully 23 times the probable error.

The total number of determinations being 7 out of the 8 made (No. 7 excluded by Stas), and the square root of 7 being 2⅔, very nearly, the probable error of the mean,

---

* I greatly dislike foot-notes, as well as crooked things generally.

But I have called attention to the *private letter of Stas* to Van der Plaats on this subject in my True Atomic Weights, p. 86, which private letter of Stas *was published* as to its main contents *by Van der Plaats* in his paper in the Annales de Chimie, VII, p. 518; 1886, as I stated with special reference to volume and page. Also Comptes Rendus, 116, p. 1363; 1893.

In Crookes' editorial of 1896 he makes it appear that I have drawn into print a private letter of Stas! It is really difficult to find words to condemn such crooks!

3 thousandths, will give us for the probable error of a *single* determination 8 thousandths, that is 0.008.

Since the mean of Stas is 14.041, half of all observed values should lie within the range 0.008 below and above this mean, that is between 14.033 and 14.049; let us say, between 14.03 and 14.05. See bottom p. 11, and pp. 16–17.

For the "fused" nitrogen atoms here considered, only No. 6 falls within this limit—the other six determinations are far above it, being located between 14.06 and 14.08.

For the "dried" atoms, a similar state of facts would result.

But why will common laborants, who like the janitors of a chemical laboratory, know of no chemistry beyond the mixing of liquids and the ignition of solids, with more or less of stink and fumes, meddle with mathematical processes they do not understand?

It is like playing with new firearms—they may find them loaded when least suspecting such a thing.

They ought to be more careful, hereafter.

II. THE ATOMIC WEIGHT OF NITROGEN BY CHEMICAL MEANS.

Really it is not necessary to enter upon the purely chemical determinations of the atomic weight of nitrogen, after the perfectly unquestionable results obtained by density determinations of Lord Rayleigh and the preceding little *Note* on Stas' Syntheses of silver nitrate and his challenge.

But the muddled state of the chemical mind, produced by the pretenses of Stas and diffused by the high chemical and academic endorsements of Stas, which have made the expression of any doubt about the Stas values a heresy, compel us to enter upon this chemical part once more.

I must be permitted to insist that a demonstrated fact, such as the atomic weight of nitrogen by density determinations, is to be received as such, and cannot be suppressed. It must be accepted as a finality.

Anything in conflict with such a fact proves itself to be in error. If the atomic weight of nitrogen obtained by strictly chemical means differs from 14, we can only look for causes of error in these methods of determinations.

But as a still further concession to the deplorable lack of a real scientific spirit in the chemical mind of the last forty years, when it was befogged by absolute faith in false methods and imaginary precision, combined with absolute ignorance of general principles of scientific reasoning, induces me to take up the determination of the atomic weight of nitrogen by chemical means entirely de novo, as if the determination by density had not been made, nor that challenge taken up.

And finally, as some of the would-be critics in high station may not be able, nor even willing, to overcome the inertia of their own mind, so thoroughly rooted in the false doctrine, as it was in an earlier period in phlogiston—I shall, in this case, resort to their own familiar method of dealing with the atomic weight determinations, singly and directly, however much inferior that method is in scientific force to our method of dealing exclusively with the immediate results of experiment expressed in the analytical ratio.

I shall, therefore, in this, the most noted case, make use of the old method of procedure by comparing directly the atomic weights immediately resulting from the individual analytical data of the laboratory work—"de la Chimie du Laboratoire." See p. 22.

In this case I shall, of course, follow the common practice of stating my own calculations with the usual degree of "precision" *of three decimals.*

If I should not follow this modern humbug and fail to give the customary imaginary decimals resulting by carrying on calculation beyond the plainest limits of precision of the experimental determinations, I would, of course, be denounced by the dominant school, or rather by the infallible church of the false prophet Stas, as lacking in the first essential requirement of modern exact science.

But finally, what shall be the auxiliary data for these calculations? Our standard atomic weights are, in this case, out of the question; for the fanatics of the church of Stas would in toto reject all results based upon our own standards and calculated by our own methods.

We, therefore, must take the data of Stas and his school. We shall take those presented with the show of highest precision, and claiming the highest authority.

These are to-day undoubtedly those of the Smithsonian Institution, declared to be the most probable by the Secretary thereof, published in fullest form at the expense of the Smithsonian fund, entrusted to the American Congress for the Increase and Diffusion of KNOWLEDGE among men "per orbem," and sent out as registered mail matter at the expense of the entire American people.

These values are, furthermore, produced in the scientific bureaus maintained at Washington by the taxes put upon the American people. In this instance, the chief responsible for this work, is the Secretary of the Interior, under whose control the Geological Survey of the United States is placed by law. The real (ostensibly the only) author is the Chief Chemist of that Survey, Frank Wigglesworth Clarke.

We shall then, for this one instance, use the final data proclaimed by the authority of the Smithsonian Institution, as elaborated in and by the Department of the Interior in one of its highest scientific bureaus, and as they are blindly accepted by the American Chemical Society, and made use of in the enormous establishments of this Government in the collection of imports, in the Department of Agriculture and the numerous Experiment Stations of this Department.

These very data, the *false* atomic weights of Clarke and the Smithsonian Institution, are now being officially forced upon the Committee revising the U. S. Pharmacopœia for adoption as standards in this work.

If these final data, used in all these government establishments of a supposed scientific or technical nature *are false*, then all chemical analyses made in these institutions, which are supported at the cost of many millions of dollars annually to the American people, *will, as a matter of necessity be falsified by these false data*—for even the best made chemical analysis will be so falsified if the data used for their reduction by calculation, are false.

With this matter thoroughly understood, I shall now proceed to the work of testing the value given to the atomic weight of nitrogen by chemical means, according to the common method of *calculating this atomic weight in every single analysis or determination directly from the analytical ratio.*

To be absolutely beyond reproach, I shall take this analytical ratio from the Smithsonian tables themselves, stating the *page* of the "Constants of Nature," of 1897, where they can be found.

All calculations have been carefully made with seven place logarithms (Schrön's, 19th edition, Braunschweig, 1881). I do not think any error has crept in; if so, I shall gladly accept the report for revision and proper acknowledgement.

The limited space, because of limited means at my disposal, and the limited time, because my age, does not encourage further waste of my time on the greatest scientific humbug of modern days, and compels me to limit this work to the *two* most famous—or as I must say, scientifically infamous—determinations, namely, those depending on the synthesis of silver nitrate, and the relation of the silver nitrate to potassium chloride.

These methods are generally regarded as the most famous by all; the first most assuredly is always represented as such, and was the very one so proclaimed by Stas himself, when he challenged the chemists of the world in the sixties and again in the eighties. The second is next in standing.

The special values, required in our reductions, we take, as stated, from Clarke, whose absurd unit we will have to use, namely, his pretended hydrogen unit, which practically means (l. c., p. 33)

$$O = 15.879.$$

The value is pretended to be affected by a "probable error" of only 0.0003.

Clarke (l. c., p. 33) says that the above value "will be used throughout this work," the Constants of Nature, 1897. So we have to use it ourselves—for this once.

May the God of Truth pardon me for basing these calculations on official lies and scientific fraud. I do so exclusively and solely to thereby prove them to be such lies and frauds, in order to destroy them and to blot them out from the face of Chemical Science which they have disfigured and disgraced for forty long years.

a. Synthesis of Silver Nitrate.

In the synthesis of silver nitrate, the analytical ratios of experiment, are given on pages 62 and 63 of the Smithsonian Constants of Nature of 1897. See also my True Atomic Weights, 1894; pp. 76–77.

We have not room to reprint them, but give the resulting atomic weights under the heading of the chemist and in exactly the same order as in the work referred to; hence absolute identification is secured, and easy reference established.

As to the calculation made, according to common practice, the following words will suffice:

The analytical ratio *a* of each determination just referred to, expresses the quotient $Ag\,O_3\,N$ divided by Ag, where O has the value above stated and where

$$Ag = 107.108$$

(l c., p. 70) with a probable error of 0.003 only. Accordingly, the numerical value of $Ag\,O_3$ is 154.745.

Since, now, the ratio *a* multiplied by Ag equals $Ag\,O_3\,N$, it follows that N is obtained by the following process:

Multiply the numerical observed value of *a*, by the given numerical value of Ag, substract, from this product, the above numerical value of $Ag\,O_3$ and the remainder will be the numerical value of N due to the determination made.

Atomic Weight of Nitrogen from Syntheses of Silver Nitrate.

For O = 15.879 and Ag 107.108.

Chemist:	Penny.	Marignac.	Stas, First,		Stas, Last.	
No.	Dried.	Fused.	Dried.	Fused.	Dried.	Fused.
1	13.874	13.877	13.941	13.921	13.944	13.936
2	.882	.843	.960	.929	.943	.928
3	.904	.877	.933	.925		
4	.885	.846	.922	.918		
5	.874	.892	.926	.917		
6	.901		.918	.900		
8			.936	.906		
Mean	13.887	13.867	13.934	13.919	13.944	13.932
[Stas, *No. 6*, above is false; true,			.899	.890	See further on.]	

This little table contains the exact atomic weight of nitrogen, as it results from each single determination made by reducing that determination in the common way by means of the final values of Clarke for the auxiliary elements O and Ag. All determinations made are taken, exactly as recorded by their analytical ratios on pages 62 and 63 of the work of Clarke of 1897.

Although modern chemists do not look at facts observed, but only at *means*, and then take these means as facts, they will please not do so in this particular case, but keep each real fact distinctly in view by itself.

And as the common custom of leaving numbers in a column, does not give the eye a fair chance to see how far these values agree—and thus the mouth and the pen may, *inadvertently of course*, proclaim as facts what is merely error or vain imagination, we shall make sure to avoid such an unhappy result.

To avoid such a deplorable occurrence of filling the record of science, and then the world, with unmitigated falsehoods and errors, we have taken the trouble to assist the eye of the mind by a simple use of the eye of the head, through plotting the above data.

In the original drawing, the hundredth of the unit of atomic weight was represented by one inch, the thousandth was, therefore, represented by the tenth of an inch, securing absolute correctness to the third decimal—in this drawing, of which a photo-reduction is printed on the lower half of Plate I.

In order to keep the individual series properly apart, each series was laid down on its own straight line, determined in place by the *mean* value of that series.

This somewhat new method of graphical representation gives a faithful and perfectly clear picture of *all the facts:* the *individual* observations as abscissæ on lines determined in place by their *means* as ordinates.

As a matter of course, the locus of the means becomes a line inclined under 45 degrees.

The results from *dried* silver nitrate are represented by open circles, those from *fused* silver nitrate by black circles.

It is evident, from the diagram, that these two conditions are thoroughly distinct; for the "dried" nitrate the atomic weight is invariably almost two hundredths higher than for the fused nitrate.

This is true for Marignac and Penny as well as for Stas.

This diagram alone must suffice to condemn the position of Stas on this question, as to which silver nitrate is the true one, the dried or the fused?

Stas and his school have left the question practically open. When confronting such contrast, they hide it under the pretention that it is of minor importance, insignificant; but in the next breath and on the next page, they claim an accuracy to the very last decimal!

This fooling the chemical authorities and through them the entire chemical world, has been carried on long enough. It is worthy of the mountebank, but not becoming the scientist, least of all when he proclaims the great precision of exact science, and challenges the chemists of the world, as Stas has done twice.

Here, in this our diagram, printed from a photographic reduction of our large scale drawing, *all* the actual facts observed are represented in space to an exact scale.

First of all, every one must admit, that the *vaunted concordance of the chemical determinations of Stas does not exist, is not a fact, but merely a boastful pretense.*

To say that Penny and Marignac differed, and that their work can not be considered in connection with that of Stas, is making an assertion that is false in fact, and when repeated after this exposition will become a wilful falsehood.

The fact is palpably evident on our diagram, Plate I, that *Stas* "dried" differs *from Stas* "fused" exactly as badly as Penny "*dried*" from Marignac "*fused*."

It is also palpably evident, that Stas, "last series" (Nos. 9, 10), differ still more from his first or older series, (Nos. 1–8; 7 he withdrew), in a direction to get away from the older chemists, Penny and Marignac.

Any thoroughly unbiased mind must take this as an evidence of *intention* on the part of Stas.

He undertook the last series to prove simple relations of atomic weights to be wrong—and he *pushed* the value of nitrogen still higher up.

These differences are *not* insignificant, for the Stasians depend upon the values of the thousandths in the claim for *exactness;* they cannot drop this when confronted with greater discrepancies between Stas' own results mutually, than between the values of Marignac and Penny.

The entire *range* of the individual determinations amounts to 12 hundredths, or 0.12, on the atomic weight of nitrogen; *that is, almost* ONE *per cent!*

The work of Stas all lies on *one* side—the work of the other chemists all lies on the other side.

This diagram shows with absolute evidence that the work of Stas is in no way more reliable than that of the other chemists.

If they erred to one side, Stas erred as much on the other side!

The *means* between Marignac and Penny differ palpably no more than the means between Stas' first and last series.

Marignac fused is 0.02 below Penny dried; but Stas fused, first series, is 0.025 below Stas dried, last series.

The school of Stas has followed the example of its founder to the letter; it has descried the work of the older chemists as inferior in exactness, therefore, practically worthless; it has denounced the ideas and determinations of other chemists as ridiculous imaginations and chimerical. See pp. 78–79; also p. 99.

Further, as to the pretended higher concordance of the individual determinations of Stas as compared to those of Marignac and Penny, our diagram representing *all* the facts *equally* and to exactly the same scale, shows this boasted higher concordance in Stas work is also nothing but a sham and a pretense.

The fact is presented in our diagram; *the divergence increases, from an approximate center, in both directions!*

Stas "fused" atoms of nitrogen differed less than his "dried" atoms, the first being placed nearer that center; so did the "dried" atoms of Penny differ less than the "fused" atoms of Marignac.

The general fact here brought out is that the further the work, not in itself admitting of exact determination, is pushed beyond its true value, in order to secure concordance, the greater will be the constant error.

By the lines A B and C D (always lower half of Plate I) limiting the Stas values, we also include all those of Marignac, for the *fused nitrate.*

In the same way, the dotted lines marked with the accented letters include all determinations on *dried nitrate,* whether made by Penny or by Stas.

The true mean of the determinations for "dried" atoms of nitrogen is in N′, taking all determinations made. Its value is 13.906.

The true mean of all determinations for "fused" atoms of nitrogen is at N, taking all determinations. Its value is evidently 13.894.

It is absurd to avoid a decision as to which of these values should be considered the most reliable.

The "drying" did not affect the "nitrate" we are made to understand. Hence, the "*fused nitrate*" is the one alone to be considered.

But what is the value of this N = 13.894 in the ridiculous units of Clarke, which we here are condemned to use?

Dividing this value by 14, we obtain the quotient 0.9924.

Dividing Clarke's assumed value of oxygen, in which all values are de facto expressed, namely 15.879, by 16 we obtain the quotient 0.9926. This is practically the same.

The mean of both quotients is 0.9925; in other words, the most reliable mean value of *all* determinations on fused nitrate of silver give for N a value exactly $\frac{14}{16}$ of that of oxygen.

The mean N′ for the dried silver nitrate is about the constant value too high, and would upon drying have come down to about N. The dotted lines on our diagram would then about coincide with the full drawn lines A B, C D passing through the heavily marked point N.

Hence, taking *all* determinations, considering all about equally well made, supposing that Marignac and Penny were good chemists as well as Stas, possibly less biased; and allowing for a proper loss of the dried, so as to take the *fused* as the true condition of silver nitrate, the existing chemical determinations give for the atomic weight of nitrogen, practically exactly the $\frac{14}{16}$th of the value of the atomic weight of oxygen used in the reduction.

Hence, the atomic weight of nitrogen, in the common way of reduction, taking all determinations, is $\frac{14}{16}$ of that of oxygen.

But we know, that the silver nitrate is *not*, even when fused, fit for accurate atomic weight determinations. Indeed, the atomic weight of silver used above, namely, 107.108 when divided by 108 gives the quotient 0.9918 which is about 7 ten-thousandths below the one resulting from both N and O above.

This shows that in these syntheses the error has been thrown on the atomic weight of silver, by Clarke and by Stas.

As to the determination No. 6 of Stas, the diagram shows the *false* values given by Stas himself, and also the *true* ones obtained by us in reducing the actual weighings exactly as demanded by the real conditions prevailing and manifest in the other determinations. That is, I do not believe that Stas made determination No. 6 at the bottom of a pit 4000 meters deep, nor in a room cooled 200 degrees centigrade below the freezing point of water.

b. The Silver Nitrate and Potassium Chloride Ratio.

This series of determinations is contained in the Recherches in the Bulletin of the Belgian Academy for 1860, pp. 290–293, and in Aronstein's Translation, pp. 306–308; it is reprinted in the first volume of the Works of STAS, issued 1894, on pp. 379–381.

By Clarke (1897), the analytical ratios are given on page 65, and the final results for the atomic weight of nitrogen on pp. 70-71. We shall here again refer to Clarke for the analytical ratios, and take his atomic weights for Ag, O, Ka and Cl for our calculations, exactly as we did for the synthesis of silver nitrate.

The data used are the same values for O, Ag, as before, to which now must be added, from p. 70 or any other

$$Ka = 38.817 \qquad Cl = 35.179$$

giving us $KaCl = 73.996$.

The analytical ratio *a*, recorded on p. 65, as observed, gives the corresponding atomic weight of silver nitrate by dividing the ratio into the above 73.996; from which quotient we, as before, subtract the value of $Ag\,O_3 = 154.745$ to obtain the value of N due to the analytical ratio found by the chemist.

Value of N from Ag Nitrate to Ka Chloride.

Chemist:	Marignac	Stas I.	Stas II.	Stas III.
No. 1	14.057	13.895	13.945	13.834
2	14.011	.907	.930	.895
3	14.014	.907	.903	.868
4	13.924	.911		
5	13.899			
6	13.926			
Mean	13.972	13.905	13.927	13.866

These actual results have also been plotted most carefully to the same scale used for the nitrate, namely, 0.01 to the inch; the reduced photo-engraving is printed on Plate III.

We note again some very striking facts by mere inspection.

First, we see that the concordance diminishes, as before, in both directions from some central value, near the heavy circle marked N on the line of means.

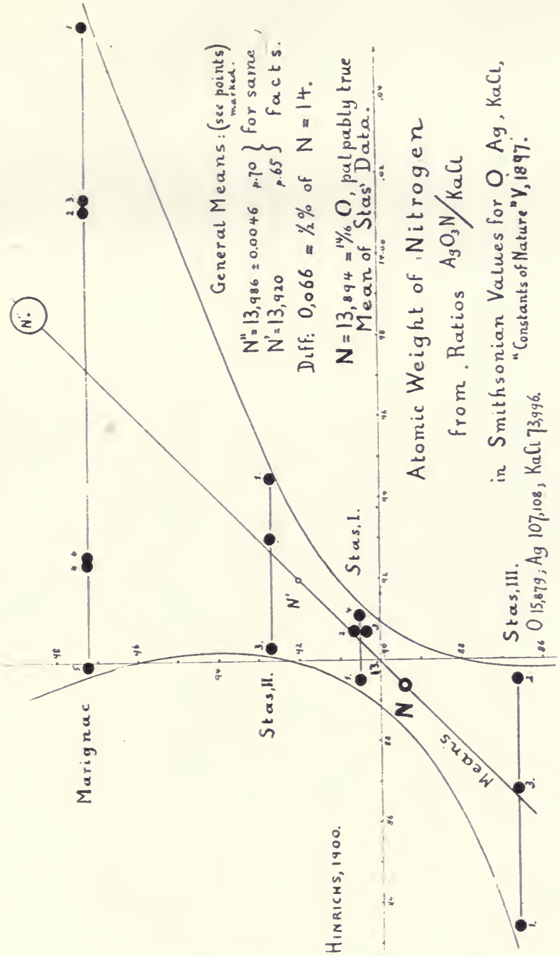
One of the most remarkable facts is readily recognized by comparing the graphics for the three series of Stas.

The first series is the most compact, the least divergent; the other two are less concordant.

Of these the second lies decidedly high, the third decidedly low.

In the original record of Stas (l. c., p. 291), we learn that Series I was made with silver nitrate, many times recrystallized, and originally prepared from pure silver.

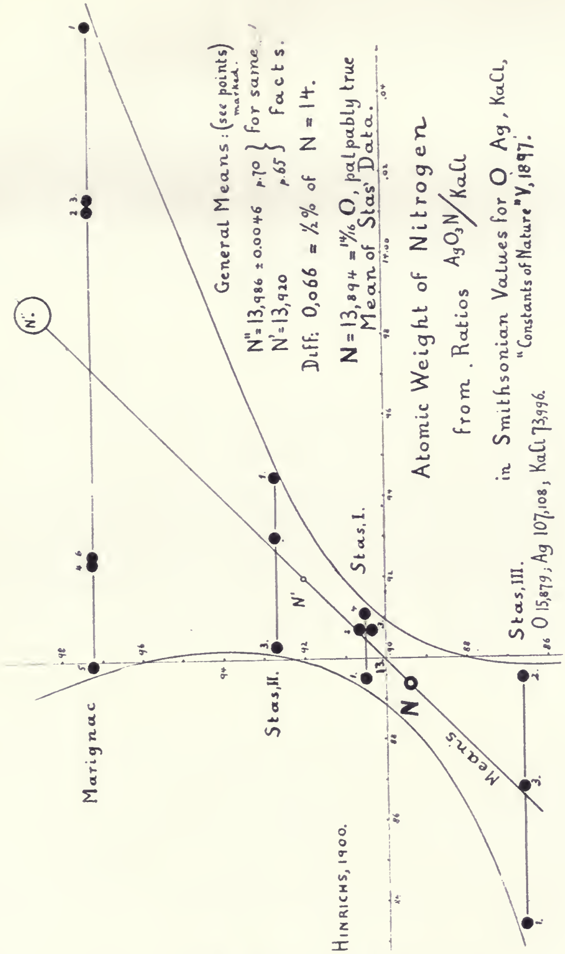
PLATE III.



STAS: ATOMIC WEIGHT OF NITROGEN.

If Stas' work were true, all dots would be grouped close together. See pp. 191-198.

PLATE III.



Stas: Atomic Weight of Nitrogen.

If Stas' work were true, all dots would be grouped close together. See pp. 191-198.

This, therefore, appears to have been *the purest silver nitrate* used by him.

The second series was made from the material used in No. 6 of the syntheses—that is, which must have been 4000 meters deep or 200 degrees below freezing point—conditions which might have affected it. At any rate, it yielded high values for N.

The third series was made by dissolving a silver deposit obtained by the electrolysis of silver cyanide in potassium cyanide.

The values of Marignac are above all those of Stas, and less concordant—the line joining all is the longest.

This is the graphical representation of *all* the *facts observed,* reduced by the Clarke atomic weights, as repeatedly explained.

Even if looking at the results of Stas alone, can any one call these atomic weights well determined, clustering around some mean value?

But if taken at their worth, if overlooking their very notable scattering from 13.84 to 13.94, that *is over a full tenth of a unit,* will not the point heavily marked N be the center (in such case of gravity, all points being equally well determined count equal in weight) for all 10 determinations of Stas?

The value for that point is 13.894; but this is again exactly $\frac{14}{16}$ of the atomic weight of oxygen of Clarke, used in these calculations.

Accordingly, if these determinations of Stas can determine anything in regard to *the atomic weight of nitrogen,* they prove that it is exactly *fourteen-sixteenths of that of oxygen.*

Hence if we take oxygen at 16 exactly, then determinations of Stas give $N = 14$ exactly. There is no getting around that.

Of course, we have here again the same trouble about the silver.

The atomic weight of Clarke, deducted from the totality of the determinations of Stas, is as before stated, 107.108;

and is somewhat less than 108 when referred to $O = 16$, namely, $Ag = 107.924$.

But as we here have to do with analytical ratios used by Clarke himself for nitrogen only, we have nothing to do with the silver value but to use it as we find it given by Clarke himself.

And in that case, as we have seen in detail, the atomic weight of nitrogen comes out exactly as fourteen-sixteenths that of oxygen—from the very determinations of Stas.

Now these determinations have for about forty years been taken to contradict this result most decidedly.

Why has not some one of the Stasian Re-Calculators noticed the fact so palpable in our diagram of the plain, actual data given by Stas himself?

This is one of the mysteries of Stas and his School, officially recognized throughout the world of chemistry for forty years.

Stas and his School denounce imagination and pretend to give facts, and exact facts only.

The fact is they play with facts, reduce them en-bloc, a whole group of them at once—so they can not tell one from the other.

The method of reduction used by Stas is like the olla podrida of the Spanish.

Throw everything from the dining table into it, as it is "left over" or obtained in the experiments; then take out as you need it, and don't mind the odor nor the error.

In the same way, but on a most magnificent, truly American scale, Clarke proceeds in his Smithsonian Constants of Nature, and has produced the most nauseating chemical olla podrida that ever was, and we hope, ever will be.

We have now twice, holding our nose, taken out from this chemical olla podrida a full set of Stasian determinations; and when considering them, without regard to anything else, only as experimental determinations made for the chemical determination of the atomic weight of nitrogen, and proceeding in the ordinary way of the art, using the auxiliary values, as furnished by the Stasian Grand

Mogul Clarke, we find N = 14 if we reduce the result to the standard O = 16.

So far as the two great series of determinations of Stas for the atomic weight of nitrogen are concerned, they give N = 14 exactly for O = 16.

But we do not need them ourselves; we have established the atomic weight of nitrogen with a much higher degree of precision by means of the weighings of Lord Rayleigh.

We do not want to use anything that has been in this Government olla podrida for five years!

c. The Meanest "Mean" and its Impossible Probable Error.

The waste of time which the Imperialistic Publication of *Clarke* of 1897, has caused me, is relieved by many remarkably fine displays of "Exact Science as She Is," in our Government Bureaus. We can only find room for a casual exhibition of such specimens as cannot be avoided in our course to establish the truth.

A couple of specially rich gems we shall have to consider here.

On page 70 of the Smithsonian "Constants of Nature" of 1897, by Frank Wigglesworth Clarke, we find for the molecular weight of silver nitrate "*three values*" from the "*general mean*" of as many analytical ratios stated to the third decimal, and characterized by a "*probable error*" to the fourth decimal.

These three values differ fully *one-tenth*, or one *unit* in the *first* decimal.

If these means for silver nitrate differ by fully 0.1, this difference will necessarily affect with its full value the atomic weight of nitrogen, obtained therefrom by simple subtraction of the constant value for $Ag O_3$.

But a range of 0.1 on 14 is about three quarters of one per cent! Rather crude results of pretendedly "exact science" that must not be questioned outside of the charmed imperial circle.

We shall here more particularly examine the third of these "general means" from ratio (3), which is the one we have just presented, namely, the ratio of Silver Nitrate to Potassium Chloride. See p. 192 and our diagram Plate III.

The "general mean," which according to Clarke, represents the data of observation given by him on page 65, is

$Ag\,O_3\,N = 168.731$, with probable error 0.0046.

By subtracting the numerical value of $Ag\,O_3$, which according to Clarke's atomic weights is 154.745, we obtain the corresponding atomic weight of nitrogen as the difference.

Simple subtraction gives us

$$N = 13.986$$

from the "general mean" of the analytical ratios determined by Marignac and Stas, given on his page 65, and represented on our page 192.

This value, 13.986, therefore, is the value from the same facts deduced by Clarke.

On our diagram, Plate III, of the facts, this value is exactly located on the line of means at the point marked N″ in the uppermost margin of our diagram; far above all the actual observations, and very far above the actual values of Stas.

This is certainly something remarkable. A "general mean" that is of so horribly and despicably mean a character as to crawl away from the observed facts of which it is the "general mean," ought to be branded "unavailable" for atomic weight determinations, the same as Lord Rayleigh's densities of true nitrogen.

Our *Chief Chemist "who lives upon Atomic Weights"* (see the statement of his lieutenant, p. 25, of our False Atomic Weights of the Smithsonian Institution) never does anything by halves, in *"exact science."*

So mean a Mean, lying far beyond the facts, especially those determined by Stas, must possess a very despicable "Probable Error."

So it does, indeed. We have above copied (from Clarke, p. 70) this "probable error" of that "general mean" located at N″ in our diagram; and *we have encircled that meanest of Means N″ by its given Probable Error.*

In fact, with a *radius* exactly equal to the probable error of 0.0046 calculated by the Exact Scientist of our Government (l. c., page 70), we have described a circle around that point N″.

As there are not quite 16 observations, all told, on our diagram, we will take the number at this maximum of 16, because we are both liberal to the Stasians, crediting them more than they are entitled to, and can easily extract the square root of that number so we all understand it, namely, 4.

Hence 4 times the probable error 0.0046 of the mean, will be the probable error of a single determination; that is 0.0184. Compare pp. 11-12 and 16-17. Let us take 0.02 for this value.

We know, that *half* of all observations should fall within this probable error of a single observation (see l. c., supra).

Hence, if we draw a circle with the radius 2 hundredths around N″ (or exactly four times the radius of the circle actually drawn around this point), this circle should include one-half of all the observed points.

As a matter of fact, not one of the observed points will be included within that charmed circle.

All points observed are from four to eight times as far from N″ as the value of the probable error of a single observation.

The laws of probability are most flagrantly belied by the results of Stas. Compare: W. Chauvenet, Astronomy, Philadelphia, 1863, vol. II, p. 488.

I must be pardoned for the expression of some contempt for the most rotten, ridiculously absurd and self-contradictory "Exact Science" of the Chief Chemist of our Government, and for the expression of regret that our Smithsonian has truly become an Institution that *de facto* increases and distributes the worst possible *errors* among men all over the globe—for these errors come in the garb of the highest, most exact of science, and are taken as truth by the people—including the members of the American Chemical Society.

Imperialistic Science can be depended upon to rival the Imperialistic Church of the past in producing, diffusing and upholding error among men "*per orbem*."

The worst crimes laid centuries ago at the door of State

Church and State Religion are no worse than the rot, rant and cant of the State Science of to-day.

They cannot call upon the stake to-day to annihilate the independent thinker; but they know how to use the more refined and more cruel methods of torture available to "*civilization*."

III. CHEMICAL ACTION CHANGES WEIGHT OF MATTER.

But we are lightly touching the "Exact Science" of our Government Institutions at Washington, including the Smithsonian Institution under the control of Congress.

The more we are condemned to take notice of the productions of the public printing press of our Government in science, the more astonished we become at the absolute lack of understanding of even the rudiments of science and logic displayed in these imperialistic publications.

There is not a chemist "per orbem" who believes that chemical combination affects the weight of matter; for he knows there is not a particle of evidence, not a solitary experiment, in favor of such an effect.

On the contrary, it is held as an axiom, not only by all chemists, but by all natural philosophers as well, that *the weight of any material particle is entirely independent of its state of chemical combination.*

But these Government "*Constants of Nature*" produced by the harmonious interactive co-operation of the highest departments and all at public expense, do declare, as a result of the "Exact Science" which has produced these "*variable constants*" that are "*contrary to nature*," that as a matter of fact, potassium chloride possesses a weight quite different from the sum of the potassium and chlorine contained therein.

I was indeed struck with awe and admiration for the Exact Science of our Government when first I noticed this great new fact on page 334 of said "*Constants*" of 1897.

I have since found that this great and marvelous annihilation of what we all supposed to be an axiom of chemistry and philosophy pervades the entire big book of Clarke on atomic weights, of 1897.

The results published as the most probable values of the atomic weights, according to S. P. Langley, the Secretary of the Smithsonian Institution (l. c., p. III) are all dependent upon this destruction of the old axiom.

If the old axiom, that chemical combination or decomposition has no effect on the weight of any particle of matter, or of any atom, is true, then the learned secretary of the Smithsonian Institution has certified to a gross systematic error that infects one of the most pretentious works on exact science which has ever been issued from that institution, which certainly was not intended by its founder Smithson, to produce or diffuse such stupendous errors.

This great and fundamental axiom of all science up to 1897, is cooly and deliberately set aside in exact weights to the third decimal on page 70 of that publication, in the following form:

Ka = 38.817, probable error, 0.0031
Cl = 35.179, " " 0.0048
. . . .
Ka Cl = 74.025, " " 0.0019

Hence, we see a change due to chemical combination, an increase of 0.029.

We must leave out of consideration such remarkable facts presented in these exact figures as that the compound is known with much greater precision than its constituent elements, etc.

We must try to grapple with the *increase in weight by mere chemical combination.*

If the entire increase falls to the chlorine, it amounts to 0.03 on 35.5 or one on 1183.

That is nearly *one-tenth of one per cent!*

How important such a chemical fact is in astronomy, both of the present world and of its probably nebulous past!

Chemical decomposition will diminish weight, and, therefore, gravitation—the celestial orbits will widen!

Again, chemical combination taking place on a grand scale in any cosmical system, the force of gravitation increasing one-tenth of one per cent, the orbits must contract.

But let us not be drawn aside from our immediate duty by the fascinating new prospects which our Government establishments of science open to our eyes in cosmos, present and past.*

We have here to deal with atomic weights only; let us return to this subject, and determine the effect of this new axiom on the value of the atomic weight of nitrogen, determined by the chemical means here under consideration.

All our calculations made and of which the resulting values of the atomic weight of nitrogen were printed above on p. 192, and represented to scale in diagram, Plate III, were, we are sorry to say, calculated upon the common supposition that *as to weight*

$$\mathrm{Ka\ Cl} = \mathrm{Ka} + \mathrm{Cl}.$$

Or, to be quite exact, in numbers, taking as we did, the atomic weights of Clarke, p. 70,

$$\mathrm{Ka} = 38.817$$
$$\mathrm{Cl} = 35.179$$

we, in our ignorance of mind and blindness of heart put the atomic weight of the compound equal to the arithmetical sum, as Berzelius would have done,

$$\mathrm{Ka\ Cl} = 73.996$$

and did not add the value of weight

$$0.029$$

"produced by chemical combination" which alone can produce the Clarkian value specifically and separately given, p. 70, as

$$\mathrm{Ka\ Cl} = 74.025.$$

For all determinations of the atomic weight of nitrogen, by means of the analytical ratio of silver nitrate to potassium chloride, in the manner fully set forth in a preceding

---

*While reading this proof, a Sunday paper brings the illustrated prediction of the greater stature of future man by Professor McGee, the official anthropologist. It is really too bad that our sensational papers get some of their most harmful errors about scientific matters from our "official scientists" at Washington.

section, we shall find the atomic weight of nitrogen 0.066 *higher* for the *higher* value of the atomic weight of potassium chloride given by Clarke as due to the compound as such, in excess of the simple sum of the weights of the uncombined atoms of potassium chloride.

In other words, we humbly confess that all our calculated values, and, therefore, all our dots on our diagram, Plate III, for the atomic weight of nitrogen dependent upon the reaction between silver nitrate and potassium chloride, are *too low* by 0.066, if the elements potassium and chlorine increase in weight to the extent given by Clarke's value, i. e., 0.029 per atom of the compound.

Hence, we found N'' at a height 0.066 above N' the true mean of all determinations, those of Marignac included.

We ought, therefore, hasten to change all these our results, *obtained by our old-fogy Berzelian notion that the atomic weight of a compound is obtained by simply taking the sum of the atomic weights of the constituent atoms.*

Surely, to commit such an error as 0.066 on an atomic weight of 14 is a *very gross error*, as it amounts to ⅔ of a tenth, that is one *fifteenth of a unit*, which is *almost half of one per cent for* $N = 14$.

It is entirely beyond possibility that, for example, Lord Rayleigh could have committed such an error, or that such an error can possibly affect our $N = 14$ dependent on his weighings.

For it amounts almost exactly to the very difference which he noticed between atmospheric and chemical nitrogen, and by which difference he was led to his discovery of argon and to the experimental establishment of the true atomic weight of nitrogen.

And I am especially sorry and most humbly confess that my statement about Stas' determinations for Silver Nitrate to Potassium Chloride, agreeing with his determinations from the synthesis of silver in fixing the atomic weight of nitrogen at $\frac{14}{16}$ of that of oxygen, was a hasty error, committed by my not noticing, in time, that the weight of the compound is different from the sum of the weights of its constituents, as demanded by Clarke on page 70 and throughout his own variable Constants of 1897.

But, upon serious reflection, I dare not even make this correction of my stupid error.

For, how can I, a poor, *independent* investigator, know without special revelation from the Exact Scientists of our National Government, how much this change in weight amounts to in the synthesis of silver nitrate?

It is very true, I have carefully copied the Clarkian values for silver and oxygen, and used the sum 154.745 to represent $Ag\ O_3$ in the silver nitrate in all my calculations.

May not this be also wrong? Although the Chief Chemist Clarke, has used that same value himself—he may ere this have discovered how much the one silver and three oxygen in silver nitrate, differ in weight from the sum of their weights in the free and uncombined state, and may it not be that he simply has not yet through the Smithsonian Press and the Journal of the American Chemical Society informed the expectant chemical world of the precise amount?

As I now, at last, come to grasp the full import of the overthrow of our old axiom of the constancy of weight of matter, irrespective of chemical combination—I think we are really entirely at the end of all possible atomic weight determinations.

If the sum of the weights of the constituents is no longer, according to Clarke, to be taken as the weight of the compound, all atomic weight determinations must cease, because they become both absurd and impossible.

Every new compound we might draw upon, would present us another unknown change in weight, and hence we would have a system of indeterminate equations.

If Clarke is right—as of course he must be, as Imperialistic Chemist for the United States of America, by position and by his assumption and by the recognition his official station secured from the American Chemical Society—all atomic weight determinations must cease, having become impossible.

This is a very sad termination for especially two reasons.

First, it will be apparently impossible for the "high authority," Clarke, to continue "to live upon this subject of atomic weights" any longer at our National Capital; see letter, p. 25, of my "False Atomic Weights of the Smithsonian Institution."

Second, the school of Stas has always proclaimed that it was Stas who demonstrated, by his experiments, the unchangeability of the weight of matter.

This we see in the great Stasian Apostle *Ostwald*, under the name of "*die Erhaltung des Stoffes*," pp. 4–5 of his Physikalische Chemie, II edition, Leipzig, 1891. On page 14 of the same work, Ostwald ascends to the declaration that *Stas worked to the ten-millionth exactly*, and asserts that *in no branch of science such wonderful accuracy or exactness has been obtained* as in these determinations of Stas.

"O come, let us worship and fall down" before this Greatest Master of Modern Science. (Venite, exultemus Domino.)

And now, all this glory of Stas, proclaimed from Leipzig University, falls to the ground by the one modest little line in our own Clarke's Variable Constants, not of Nature—that Potassium Chloride weighs appreciably, yea very considerably, more than the sum of the weights of its constituent elements.

How sad my kind and good friend Ostwald will be when he learns of this terrible ending of all fixed, definite proportions in chemistry, of all attempts at determination of atomic weights, and that all the glorious precision of even his own Great Master Stas, was nothing but a mere shadowy imagination. "Vanity of Vanities."

It is true, Ostwald has close at hand this great work of Clarke on the "Constants of Nature."

Ostwald has indeed "*reviewed*" this work of Clarke—but he has not done the work justice, has not studied it properly, for he does not mention this, the most striking and astonishing discovery of Clarke.

Herr Geheimrath Wilhelm Ostwald, of Leipzig, should take that work of Clarke of 1897, produced by our *National Government and its Scientific Institutions, which are the most costly on earth, containing the most despotic and fanatic scientific (?) men on the globe,* and study it again and again, and then study it more carefully still.

He would then find that his declaration (Zeitschrift, Bd. 23, p. 187; 1897) was too hasty—namely, that the estimation of value or weight of determinations used by Clarke, is *nonsense* (hat keinen Sinn); is simply *Furor Clarkii.*

When, upon such more careful reading of the work of Clarke, our gentle friend Ostwald, verifies my recognition here given of the annihilation of our common foolish notion of the constancy of weight, Professor Ostwald will beg Clarke's pardon and acknowledge meekly his stupidity and error, as I have done above.

And then, Professor Ostwald and myself, will as two penitent brothers, join hands, and feel happy that the Great American Nation maintains at the cost of many millions of dollars a year, stupendous scientific Institutions, Bureaus and What-Nots, in which *the most eminent scientists have been living on atomic weights,* until, at last, these atomic weights have given out.

Lavoisier is pointed out by Kopp (in his Geschichte der Chemie, II, p. 73) as the chemist who first made a formal statement of the *indestructibility of matter*—or to speak more scientifically, *die Erhaltung des Stoffes.* The old Greeks had a notion of that sort, but that does not count before exact chemists.

This, retained by chemists till the present, implies that the weight of a compound is equal to the sum of weights of its constituents. Berzelius never doubted this axiom, but based all his work upon it.

This pretended axiom has been demonstrated to be false by Clarke in his famous *Constants of Nature,* edition 1897, on pages 70, 108, 324, 334 formally, and throughout the entire book in all its final results.

With this grand discovery of the Chief Chemist Clarke, chemistry of precision suddenly terminates in a sort of RAGNAROK that must involve the *Constants of Nature* and the *Workshop* where these constants were manufactured, and the great Manufacturer of Atomic Weights himself.

Sic transit gloria mundi.

IV. HERESY IN THE CHURCH OF STAS.

It is surely bad enough for our great chemist of the United States, the Chief Chemist of the Department of the Interior and of the American Chemical Society, to ruthlessly destroy our old faith in "*die Erhaltung des Stoffes*," which was transmitted to us through our Chemical Saint Lavoisier from the Greek Sages; but for Frank Wigglesworth Clarke to ignore the highest chemical authority of Berlin is too much for me to stand without some action.

In addition to this scientific reason, I have also a personal reason to feel the insult to the great official German Chemist; for it was the abandonment of Schleswig-Holstein by Prussia, after having urged us poor peasants on into war against Denmark, that brought me to Copenhagen, and later, when the appetite for our land became whetted in Berlin, made it necessary for the German-born to pull up stakes and go to Egypt. Really, I sometimes feel as if I had been sold by my German brethren, as was Joseph of old by his brothers.

Der Herr Geheime Regierungs-Rath, *Hans Landolt*, first Professor of Chemistry of the University of Berlin, has demonstrated experimentally, that chemical combination has no sensible effect on the weight of matter. His experiments are much more delicate than even those of Stas; for all weighings are given, in print, *to the thousandth of a milligramme*, while Stas did never go below the tenth and then lumped it by 12 to 66 milligrammes, when necessary. Besides Landolt's individual experiments extend over a long period of time—up to several years.*

Der Herr Professor Hans Landolt has presented (vorgetragen) his results at the meetings of the Royal Prussian Academy of Sciences, at Berlin, on March 12, 1891, and on February 4, 1892. The entire research is published in the

---

* *Erste Reaktion* from October, 1890, to March, 1892.

Sitzungsberichte der Kgl. preuss. Akad. der Wissenschaften, for 1893, pp. 301 to 334. This is really the original publication, and should properly be given as the original source whenever this research is referred to.

It is disgusting to find this great work published in full in the Zeitschrift of Ostwald, Leipzig, without any reference to the Academy which has published it in its Transactions. The one great reason for the support of such academies is found in the publication of their Transactions. When such plebeyan editors as Ostwald can publish such researches as *original contributions* to their journals, the very existence of the great academy of science is being undermined.

The United States Patent Office also robs the Sitzungsberichte of the scientific discoveries of Professor Emil Fischer, assignor to C. F. Boehringer & Soehne; elaborate formulæ and all.

Not enough that Editor Ostwald publishes these great researches, even in advance of their official publication by the academies concerned, the chemical public is systematically kept misinformed about these original publications, so that the "Jahrbuch" and in fact practically all chemical records, ignore the academies, and exclusively refer to the piratical (?) journals.

Thus the great research of Landolt is commonly credited to Ostwald's Zeitschrift (Bd. 12, pp. 1–34; 1893); also to Berichte d. D. Chem. Ges. 1893; 26, 1820. I have not seen *one* publication referring this research to the true source, the Sitzungsberichte of the Academy of Sciences of Berlin. Of course, I have dutifully given the proper reference, see True Atomic Weights, 1894, p. 39, where the remarkable opening sentence of Professor Landolt is quoted in English translation.

This same Editor Ostwald has acted in the same bad spirit toward the Academy of Sciences of Copenhagen by printing great researches presented by Julius Thomsen (in Danish) to this academy as original contributions (in German) to his Zeitschrift für Physikalische Chemie, and issuing this German paper long before the Danish original appears in the *Oversigt*.

How Editor Ostwald gets such manuscripts out of the archives of these academies is quite remarkable, and ought to be investigated.

Now this truly wonderful chemical research of the highest chemical authority at Berlin—and consequently of the world—is absolutely ignored by our Chief Chemist; the name *Landolt* is not even contained in the index to authorities of the Constants of Nature of 1897.

And yet this research of Landolt deals exclusively with die Erhaltung des Stoffes. By this research it is experimentally demonstrated that chemical action is totally without influence upon the weight of matter itself. The limit of precision has been carried to the very utmost in these researches.

Herr Professor Hans Landolt is, of course, thoroughly convinced of the extraordinary exactitude of the work of Stas—he begins and ends with emphatic declarations to that effect.

Now, here it is where ordinary chemists are placed in a dilemma.

Both the greatest chemical authority of Europe and the greatest chemical authority of America, are firm and profound Stasians; and Stas himself used all his skill to produce "*total syntheses*," which rest for their demonstrative force on die Erhaltung des Stoffes.

Yet, without even referring to this great work of Landolt, Clarke completely ignores that principle and, with true and most becoming modesty, quietly used his own grandest discovery of modern science, namely, that "mass" or "weight in a given place" *does* depend upon chemical combination.

Clarke has not even mentioned this, his most wonderful discovery, *in words*—he has simply stated it in numbers, as a fact.

Si Quæris, Circumspice!

Like that great Architect Wren, of England, he seems quietly to wait till the chemical world shall look about; it will then recognize his most astounding discovery.

And it is magnanimity which has prevented him in 1897, from referring to Landolt's research of 1893. If he had mentioned Landolt, he would have been compelled to say that Landolt's "point of view is so radically different from " mine that I have been unable to make use of his discus- " sions." Constants, p. 6.

Or, perhaps, he would have regretfully remarked, as he does (p. 60) about Lord Rayleigh's experimental determinations: "the research of Landolt is unavailable for any discussion of atomic weights. Perhaps, at some future time, the figures of Landolt may be so corrected as to be useful in atomic weight calculations."

I am sorry to see the American Stasian Clarke find it possible to demonstrate, by the glorious concordance of all the determinations of Stas, that chemical combination changes the weight of matter, while the German Stasian glories in having confirmed the fundamental result of all work of Stas, that chemical combination has no effect whatever on the weight of matter.

Indeed, it would give me unspeakable joy if these highest chemical authorities of Europe and America and both high Government Scientists—although not yet both de facto Imperial Scientists—could as faithful disciples of the one and only Exact Chemist, Stas, unite and agree on what it is that Stas has established with such wonderful precision.

What proportions this Schism in the Church of Stas will assume I dare not contemplate. I shudder.

Both these greater authorities are fanatic disciples of Stas; both declare all question about *the exact commensurability of the atomic weights definitely settled* BY STAS, and against the possibility of such commensurability as N : O = 14 : 16 = 7 : 8 *exactly*, or C : O = 12 : 16 = 3 : 4 *exactly*.

See the quotation of the opening sentence of Landolt in True Atomic Weights, p. 39, "Bekanntlich — —"

Of course, both these greatest Apostles of Stas stand upon the solid ground of experiment—from the hundredth to the ten-thousandth of a milligramme! Both of these leading Apostles of Stas are destitute of and abhor imagination—of any general principle of absolute science (mathematics) or old fogy belief in the highest truths of a Pythagoras or a Plato.

Our showing up of their fancied weighings to the thousandth of a milligramme by the simple use of their pencil and paper, is of course nothing but what might be expected of such a heretic barbarian who dares think truth higher than authority of position or decoration.

However, to see the Church of Stas split upon the old rock of *die Erhaltung des Stoffes*—the permanence of matter in weight—is enough to sadden even the heretic, who now must fear they will let the fires go down and thus not burn him quickly at the stake, but just slowly smother and smoke him.

Here endeth the Reductio ad Absurdum.

C. THE CONCLUSION.

Our Standard Atomic Weights have been Proved to be the True Absolute Atomic Weights of the Chemical Elements.

We shall not carry this work any further; we shall rest our case here.

Those who still may claim to be not fully satisfied, we shall not trouble any further with reasons or facts.

Our standard atomic weights gave us the numerical values of the standard atomic weights of all the compounds used in atomic weight determinations.

A simple division then gave us the standard atomic ratio, which we calculated to five decimals.

This atomic ratio was taken as standard of comparison for all analyses made, each one of which was expressed by its own analytical ratio, also calculated to five decimals.

The comparison of these analytical ratios, representing the observed facts, the chemical analyses, showed throughout as near an absolute coincidence with the calculated standard atomic ratios as the degree of actual precision attained to would allow.

Within the degree of precision attained in the quantitative analytical work done in the best laboratories by the best chemists of the nineteenth century, the coincidence was perfect.

That proves the statement made in general terms at the head of this closing chapter.

The very fact that practically all modern analysts have done their work as opponents to the law affirmed and established by us upon the very experimental determinations of these chemists, gives a greatly enhanced value to our demonstration.

Let us for a moment turn to the excellent laboratory work of Crookes, taken by himself as absolutely and permanently establishing the absence of any commensurability in the atomic weights of chemical elements.

It will be remembered that every circumstance and feature of this laboratory work, when freed from errors and false data of reduction, proclaims the conclusion placed at the head of this chapter.

The Maxim of Chee in Lun-Gnee.

There is one accusation of "selection" made by Crookes in his denunciatory editorial of 1896, to which I had written a lengthy and naturally a very caustic refutation; but I have withdrawn this very interesting and entertaining article, and shall simply make Crookes feel the force of the general argument, given as closing part of this work, without reference whatever to Mr. Crookes individually.

If Sir William Crookes is able to understand, he will understand, as one of my readers. If he is not able, or perchance still unwilling to understand, it does not seem necessary for me to take any notice thereof.

I shall, in this matter, follow the old maxim of the Chinese Sage CHEE, given in I, 8 of Lun-Gnee:

"Who do not strive to learn, to them I do not unfold my
"ideas; who open not their minds, those I do not instruct.
"When I describe one corner, if the pupil comprehend not
"the other three, I do not repeat my instruction."

Having mislaid the original, I beg the readers to be satisfied with the translation. For a while I studied the Chinese language seriously.

The only modification we have to make to this excellent maxim to fit it exactly to this chemical question, is in regard to the precise number of corners.

Modern chemists, being all infected by the tetrahedral bacillus (Gen. Chem. 86, 1–6; 1897) will require six corners of the full form, the octahedron.

We have only presented one of these corners in the atomic weights of the thirteen elements considered.

But there are probably six times that many elements.

In other words, we have only described one corner, out of the six of the chemical octahedron.

Is Our Demonstration General?

Is our demonstration, given for 13 elements, sufficient to cover six times that number, or all the chemical elements known, and even all not yet known?

Let us examine this question with such care as it deserves. Those versed in the remarkable accumulation of evidence by compliance with a most special condition, imposed upon each one of a number of individuals, such as we have discovered in the cases examined, need no further demonstration.

However, it may even to these readers prove interesting to obtain a numerical valuation of the force of this evidence.

The elements made use of so far, are the following 13, comprising the most important and best investigated of all:

Lead, Iron, Mercury, Sulphur, Chlorine, Carbon, Calcium, Magnesium, Platinum, Thallium, Boron, Sodium, Nitrogen.

The experimental data used in our determination were furnished by the following eminent chemists in the order of time:

Berzelius, 1810–1830; Pb. —Turner, 1833; Cl. —Dumas, 1840; C. —Erdmann and Marchand, 1844; Hg, S, also Ca. —Svanberg, 1844; Fe. —Scheerer, 1850–57; Mg. —Crookes, 1873; Tl. —Seubert, 1881; Pt. —Ramsay and Aston, 1893; Bo, Na. —Lord Rayleigh, 1895; N.

This list of names comprises none but first-class chemical analysts and experimental philosophers. The period of time actually covered is 85 years, from Berzelius, 1810, to Lord Rayleigh, 1895.

The Work of Four Generations of Chemists.

Some of the biggest fools put by mysterious powers into positions of influence for bad as well as for good have exhibited with glee the enormous dimensions of their ears and enjoyed the echo of their bray, saying: "Hinrichs has not made any new experimental determinations."

Could any one individual do the work here required that has been done by four generations? Compare True Atomic Weights, 1894, pp. 201–204.

It is true, Stas and his school, have studiously and steadily created the opinion that these great chemists were mere Tyros compared to Stas; but we have shown how false this opinion is.

The great work done by these Master Chemists has, however, thus far, not become properly useful to science, because it has not not been properly reduced.

The present condition of this great experimental work is worse than that of the observations of Tycho Brahe, made at Uraniborg, at the time he was driven out of Denmark. True Atomic Weights, p. 54.

The reduction by Kepler made the observations of Tycho most useful to astronomy.

It is that work it has been my ambition to do for chemistry in regard to the atomic weight determinations of the last century.

The Probability of our Conclusion.

The limit of precision or accuracy, for a number of these determinations is as high as 0.001 at least. Such are the atomic weights of Bo, C, Tl, N. For less than this number the limit attained does not quite reach 0.01.

We term a precision "high" when the limit of the final possible error is small.

To make our demonstration as strong as possible, we will suppose the degree of precision to be 0.01 only in all cases, and count only 12 of the thirteen elements we actually have.

On another occasion we may take the full value of all; here we desire to give every advantage to the other side.

If we limit the precision to *one hundredth* only, we have 50 such on either side of the atomic weight terminating .00 (to the hundredth exactly). Half of these are negative, the others positive; in all, one hundred distinct possible values.

Hence for any one element, the value terminating with .00 is just one in one hundred possible ones.

For *two elements*, any one of these 100 decimals might be combined with every one of the 100 of the first; hence, the *total* number of *possible* combinations of such decimals is 100 × 100 for two elements.

Only *one* combination out of these 10 000 equally possible ones is the one in which both atomic weights terminate in .00.

It will be readily seen, that for *twelve elements*, the chance that all twelve atomic weights terminate .00 is only one in 100 raised to the 12th power, that is

as 1 to 1 000000 000000 000000 000000.

In case we had counted the thirteenth element in, this number would have been hundred times as large, and contained 26 ciphers.

For every element for which the precision reaches the thousandth, we would have gained one additional cipher in this big number.

We see that we could have insisted on a million times as large a number as the one above given.

But I think the above number is large enough for our purpose of demonstration. Let us try to express the result in words.

The chance that all the twelve chemical elements have their atomic weight, expressed to the hundredth of a unit, terminate with two ciphers, is as ONE *to the number*

1 000000 000000 000000 000000.

Since now the twelve elements specified actually do so, notwithstanding this extraordinary minute chance, they do so because it is a *Law of Nature.*

We have not referred specially to the few cases (at most 5 of 50 fairly known) which terminate in .50 instead of in .00, as do chlorine and copper.

In this case, the demonstration remains the same exactly, provided to either side there is the same distance of half a unit to the beginning of the next; in other words, the next full number must be distant 1½ full units either way.

We may also express this condition by saying that where an *exact half* atomic weight occurs, we can only have *one* element in an interval of *three* units between the neighboring two.

Such is actually the case in every instance; in fact ordinarily the distance is even greater. Thus S 32 and K 39 are the nearest elements in atomic weight on either side of Chlorine, 35.5.

Why we did not "Select" the Elements.

The possibility that the coincidence established by us as a fact, might be a mere accident, is absolutely none.

Now, we did *not select* the twelve elements; and if any one were to try it he would have 1 000000 000000 000000 000000 against one to fail. He would fail—unless it were a fact of nature; a law!

No man of any mathematical sense would think of such a possibility of "selection."

To find a single needle in a haystack covering the entire United States, would be a mere child's play compared to such a selection.

I really suppose that even Sir William Crookes, great expert as he is supposed to be, would not undertake to select or find a needle in such a haystack, which I beg him to believe, would be *several* times* as large as all England and Wales, with even Scotland and Ireland thrown in.

---

* Fully thirty times.

Find a Needle in that Haystack.

When dealing with a number so vast as we have here obtained expressing the certainty of our final conclusion, it is extremely difficult to convey any sort of an adequate idea of the real force of the argument because the number itself lies beyond the conception of the human mind.

We shall, therefore, interrupt our argument for one moment longer, in order to develop this illustration of "finding a needle in a haystack" in the manner already indicated.

Indeed, this conception of the practical impossibility of finding a single needle lost in hay stacked up, is the most common and striking mental picture in our language expressive of a chance that is practically zero.

Let us consider an ordinary rectangular haystack having a base a rod square. Then an acre will hold 160 such stacks, and since a square mile is 640 acres, it will hold 102 400 such stacks. Let us say 100 000 stacks to the square mile.

The United States (including Alaska) are 3.6 million square miles, and would, therefore, hold 360 000 millions of such haystacks.

To find a needle in the haystack covering the entire surface of the United States and Alaska, would, therefore, be to the finding of a needle in a common haystack of a square rod base as

1 to 360000 000000.

We see that this number is a mere handful when compared to our own number above given.

If we extend the haystack to cover all America, both North and South, its base will be 16.3 million square miles, sufficient to hold

1 630000 000000

such haystacks of one rod square.

Also this number is insignificant when compared to the one expressing the possibility of the " selection " insinuated against us by a "high chemical authority" who likes to rush into editorial print to show his utter ignorance of what he is talking about and " denouncing."

Let us extend our haystack to the whole land surface of the globe, which is estimated at 52.5 million square miles, and it will hold only 5 250000 000000 such stacks.

To find a needle in a haystack covering all the land of the terrestrial globe, therefore, is a mere child's play in comparison to finding the one chance in our number given above.

The entire surface of the earth, land and sea, all counted in, amounts to only 200 million square miles. A needle in a haystack covering the entire surface of the earth will, therefore, be

as 1 to 20 000 000 000 000,

which chance is

50 000 000 000

times *greater* than the one under consideration above.

Now then, if to find a single needle in a haystack of a square rod base and say about a rod high, is a chance of say

1 in 50 000,

then the "selection" of 12 elements to be successful in the sense above given is

one million times

more difficult than finding a single needle in a haystack covering the entire surface of the earth, both land and sea.

In other words, our haystack must be a million times as large as the entire surface of the earth.

Taking all the planets of our solar system, we obtain only a total surface of about 160 times that of our earth. Even the sun has only a surface of 11,700 times that of the earth.

The combined surfaces of sun and all planets, therefore, is less than 12,000 times the surface of the earth.

The haystack covering the surface of all bodies of our solar system, gives us less than the eightieth part of the area required for the haystack to contain the single needle which to find will be equal to the chance of our twelve elements having atomic weights terminating in .00 exactly.

For this, our haystack, we need a globe having exactly one thousand times the linear dimensions of our earth. A town lot of 50 by 100 feet on our earth would represent a surface about equal to two congressional townships on this new globe.

A river valley three miles wide on our earth, would be like the Atlantic, 3,000 miles wide.

A little town of one thousand inhabitants on our earth would be represented by 1,000 millions of inhabitants.

The entire sun would be only one-tenth in dimension, one-hundredth in surface, of this required globe.

To find a single needle in a haystack covering this globe, the surface of which is a million times that of our earth, is exactly the same chance as that the atomic weights of twelve elements are full numbers to the hundredth of a unit exactly, by chance.

Now, as these twelve elements actually do so terminate in fact, this fact is *not* a matter of chance, but due to a Law of Nature.

This is the best I can do to give the reader any conception of the meaning of the chance expressed in the number above given, that unit followed by twenty-four ciphers.

Why our Demonstration Applies to All Elements.

But since we have not *selected* the dozen elements, except for the fact that the analytical work done was the most perfect (such as done by Berzelius and Crookes, by Ramsay and Lord Rayleigh), then this calculation applies to *any* twelve out of the total number of elements.

Accordingly, this calculation does apply to all the chemical elements!

The mathematical expression of this great natural fact, may be stated in the following words:

The atomic weights of all chemical elements are exactly commensurable;

The greatest common divisor of all is the twenty-fourth part of the atomic weight of diamond-carbon.

The Atomic Number.

If then, we take this weight as unit and call it the *atomic weight of pantogen,* the atomic weight of all chemical elements will be some definite natural number, expressing the number of pantogen-atoms having the same weight as the atom of that element concerned.

These numbers we might call *atomic numbers;* or in German, *atomzahlen.*

From what was incidentally stated in regard to the termination .50, it follows:

The even atomic numbers are at least ten times as numerous as the odd atomic numbers.

The fact is all we care for here; the meaning shall be developed on some other occasion.

The Honorable Secretary of Berlin.

Such a table of *atomzahlen* I sent, almost fifty years ago, to the Hon. Secretary of the Physical Society of Berlin, Professor A. Krönig. See Programme der Atomechanik, Iowa City, 1867; p. 3; also True Atomic Weights, 1894; p. 3.

In 1863 this same German Professor published *this system as his own** in a text book on chemistry and also pp. 53-60, in his "Neues Verfahren" against Liebig.

Such a table of *atomic numbers* we obtain by simply doubling our standard atomic weights.

This evidently implies the *Unity Matter* or the composite nature of the chemical elements, and their resulting from the condensation of one single primitive substance, which we have called PANTOGEN.

Thus, an atom of hydrogen consists of 2 pantogen atoms; C of 24, O of 32, Hg of 400, Pb of 414, Fe of 112, S of 64.

We shall not enter upon this subject at this place. See Part III of our True Atomic Weights, pp. 205-256; 1894.

---

* If the brutal editorial of Crookes, in his Chemical News of May 15, 1896, has any meaning at all, it endorses this "new proceeding" on the part of the Secretary of the Physical Society of Berlin; but most people will call this act of Dr. A. Krönig a most infamous kind of a steal.

For some years, I actually supposed that only among German Scientists such moral scoundrels could be found.

I have since learned, by experience, that I was mistaken in this opinion.

PART FOURTH.

Tabular View of the ATOMIC WEIGHT ANALYSES Of the Nineteenth Century.

INTRODUCTION.

It was our intention to give a complete summary of all determinations made during the nineteenth century. See page 85.

But this work has assumed considerably larger dimensions than anticipated.

It is also of the utmost importance to limit it in size so as to make it possible to secure a wide circulation demanded by the subject and the object.

We have, therefore, culled our complete set of cards and omitted all really worthless determinations, as well as repetitions.

Thus, the work on lead, Part Second, is simply referred to, but not tabulated again.

Also, student's work on hydrogen generation by zinc, it has not been deemed necessary to give, as it was scientifically worthless (see under Zn).

In one place the chemical work was so inspiring, that we improved upon Heine and left out many worthless figures.

The order of arrangement is alphabetical, after the chemical symbol of the element, which order we find most convenient for chemists.

A few elements have found no record at all; no serious work of permanent value could be found.

The non-valent elements have been omitted, for the present time. Theoretically they are most important; see General Chemistry, 1897, pp. 380–381, and Principles, 1874, pp. 180–181.

These non-valent elements will be fully considered in a contemplated special work on the unity of matter.

In several places we have made use of a contracted form of tabular representation, as already done, page 100.

We found it sufficient to give, *first*, our absolute atomic ratio; *second*, the chemical formula of the substance and product actually used; *third*, the extremes and range, and *fourth*, the analytical excess of the *mean*.

It will readily be seen that these four particulars really are fully sufficient and imply all the details essential.

To a few elements we have devoted more space than may seem proper; for example, to arsenic. But on careful reading, it will be found that nothing could have been omitted without real loss to the subject.

This record furnishes an excellent indication of analytical work urgently needed.

But in order to be available, the fields thus indicated must be worked thoroughly and conscientiously.

We must cease to run hobbies, to follow routine; we must again be severe in the choice of methods, as in the first half of the nineteenth century.

The methods of the school of Berzelius must be revived, and checked by our methods of calculation and criticism.

At the same time the severity of our requirements are revealed in our finding it impossible to assign a definite atomic weight for several elements for which numerous determinations have been made, all more or less conflicting.

Thus, for palladium the value 106 has generally been accepted, and 106.5 seems to result from Keiser's determinations. But these results are probably all too high.

Upon most carefully revising the determinations at hand, for the completion of this fourth part, I had to drop several elements for which the standard atomic weights seemed reasonably well determined, such as Pd, Sr.

We need thoroughly independent analytical work for such elements. Keiser is too much given to "confirm" what is current, and Richards is too much in the wet way, to attach real value to their determinations for these metals.

Ag = 108. SILVER. MAUMENÉ, 1846.

The true atomic weight of silver was determined more than half a century ago, by *E. Maumené;* his admirable determinations were published in the Annales de Chimie et de Physique, T. 18, pp. 57–61; 1846. See also Sebelien, pp. 81–82, and especially my True Atomic Weights, 1894, pp. 195–199.

The *silver acetate* he prepared in "very beautiful crystals" (Nos. 1, 2); of which a large lot he recrystallized with extreme care for determinations, Nos. 3, 4, the purest he was able to obtain. No. 5 was made on a small scale from silver chloride.

The *process used* in the determination is analytically and atomically the sharpest we know; namely, by combustion, the carbon is determined as carbon dioxide, as in the noted combustion of the diamond by Dumas, while metallic silver remains as residue; in this case without loss by volatilization, the combustion being effected at a comparatively low temperature.

The presence of a trace of occluded water in these crystals will have no influence on the result, as is perfectly evident. This was one of the determining reasons for selecting this process of analysis.

It is passing strange that this in every manner excellent work has been almost ignored for half a century; in Clarke's Constants, the weighings are given, but the *spirit* is omitted, hence the record is a barefaced misrepresentation of the real facts, as we shall see.

Silver Acetate, $Ag. C_2 H_3 O_2 = 167$.

$2 C O_2 : Ag = 88 : 108 = 0.81\ 482$. Change 76 low.

Subst.	No.	$C O_2$	Ag.	Analyt. Ratio.	Excess.
B.	1	6.585	8.083	0.81 467	15 low.
"	2	9.135	11.215	453	29 low.
Purest Acetate:					
A.	3	11.693 5	14.351	482	0
"	4	7.358	9.030	484	2 high.
Other Acetate:					
C.	5	16.475	20.227	451	31 low.
				Mean, Subst. A., 0.81 583	1 high.

which corresponds to Ag = 107.9987.

Silver Oxalate, $Ag_2. C_2 O_4 = 196$. Same process; burned quietly when mixed with pure sand.

$C O_2 : Ag = 44 : 108 = 0.40\ 741$. Change 38 low.

No.	$C O_2$	Ag	Analyt. Ratio.	Excess.
1	5.835	14.269	0.40 807	66 high.
2	7.217	17.754	650	91 low.
3	4.703	11.550	919	22 low.
Purer Oxalate:				
4	4.387	10.771	730	11 low.
5	3.533	8.674	731	10 low.
Purest Oxalate:				
6	4.658	11.535 5	734	7 low.

Clarke gives these six determinations under *one* heading, without discrimiation; but a glance at the result would show any one that the first three are entirely different from the last three. Not having considered these oxalate determinations of equal importance with the acetate determinations, studied by me in the original publication for my paper, printed pp. 195–198 of my True Atomic Weights, I cannot now refer to the original.

But Sebelien gives all the data we need, pp. 81–82. He says in *three* determinations Maumené noticed some red fumes, the oxalate having been obtained by precipitating the nitrate by oxalic acid; hence these first three are not even considered by Sebelien.

Maumené, thereafter, precipitated by ammonium oxalate, and put a layer of metallic copper after the charge. This gave him 2 determinations (our Nos. 4, 5), which, however, still showed a faint trace of the red fumes. Hence he started with acetate, which after most careful purification was precipitated by oxalic acid, giving him the *purest* oxalate for *our No. 6*.

Sebelien, as stated, does not give Nos. 1, 2, 3 at all; his *No.* 1 is our No. 4, and his No. 3 is our No. 6.

We shall take *all* determinations, for they are all equally *instructive* if we have regard to the established degree of purity of the substance.

Nos. 1–3, contaminated by a trace of nitrate, give greatest analytical excesses, both high and low, averaging 16 low. These determinations were considered by Maumené as merely preparatory.

He detected the error, and eliminated it, by the improved mode of preparation stated.

The results (Nos. 4, 5) are *good;* but he prepared a sample still more pure and got (in No. 6) the best that this method of analysis can produce.

We see here (as well as under the acetate) how dreadfully misleading are even the *statements of fact* in this Smithsonian Publication of Clarke. His ratios are also given to 4 places only.

Indeed, even as a record of experimental data, supposed to have been *intelligently* copied, we are forced to discard this work; manifestly not even that much intelligence was expended upon its preparation, to furnish a true copy of the actual results, which of necessity, implies the statement of the greatly varying degree of purity.

The record, as here given by us, exhibits in the analytical excess, a most striking demonstration of the gradual approach to the truth as purity of substance is secured, and gives also an excellent testimony of the *intelligence of Maumené* in skillfully approaching perfect purity.

The record, as given by Clarke, would condemn Maumené as analyst. We do not say that Clarke intended to create that impression; really, he probably did not understand the subject at all.

Now, for 7 low with the purest oxalate, what would be the corresponding atomic weight of silver? Since 7 is *less* than $\frac{1}{5}$ of 38 low (for 0.1 high), the atomic weight of silver corresponding to this analysis is less than 0.02 high, or *Ag is less than 108.02*.

The oxalate process is necessarily inferior to the acetate process, which in 1894 I used alone; but the oxalate process is a good approximation and thus offers a valuable and instructive confirmation.

But even the oxalate process of Maumené is much superior in accuracy and chemical reliability to the "famous determinations" of Stas, which superseded all good chemical work, but will now soon be remembered only as infamous impositions.

There can remain no possible doubt about the atomic weight of silver; it is found within 0.001 to be 108. It is 108 *exactly*.

Other Determinations.

Organic Silver Salts weighed, and silver obtained by reduction, weighed. Loss of silver almost unavoidable.

Liebig and Redtenbacher, 1840, made 5 determinations each on the acetate, tartrate, racemate and malate; results reasonably concordant, but all means 60 to 67 low (red. to vacuum), representing the atomic weight 2 to 3 tenths low (107.7).

Maumené, 1846, did considerably better work on the acetate; a third recrystallization gave him only 5 low, corresponding to 107.975.

It was upon the material furnished by Liebig and Redtenbacher that *A. Strecker,* in 1846 (Liebig's Annalen LIX), for the first time inflicted a purely mathematical curse upon chemistry, by supposing *no* errors but absolutely uniform and constant ones (how utterly absurd in chemistry) and then applied the formulary of elimination and the "*Method of the least Squares.*"

The process and publication was most learned, the results palpably false. See a summary, Sebelien, 73–75.

Clarke applied this same process to cadmium sulphate.

Essentially the same process is underlying the work of Stas; the method of the least squares, was most fully applied by our own Clarke, though Ostwald and Van der Plaats went quite far enough into that labyrinth of error.

But when chemists are fool enough to take the *Mean* as very nearly the true value, and the "probable error of the mean" as about the distance from that mean to the true value—we can expect anything absurd.

Hardin, 1896, has subjected small amounts of acetates and benzoates to electrolysis. His mean for the first is 34 low, corresponding to 1.6 tenths low or 107.84. This corresponds exactly to Stas, for the true $N = 14$. Possibly selection of results was made, using Stas as standard; Hardin has admitted practicing selection (see p. 100) and fears the oracle at Washington.

Silver Nitrate, Chloride, and the entire *Mixtum Compositum* of *Stas,* 1860–1882, wet and dry, must be definitely placed in *die Chemische Rumpelkammer,* as we have shown with sufficient detail in this work and in our True Atomic Weights, namely, pp. 70–138.

Methods that have been demonstrated to be fallacious, must be set aside—and thrown out from Chemical *Science* forever.

Al = 27. ALUMINUM.

Ammonium Alum, $Am_2 Al_2 (O_4 S)_4 + 24 H_2 O = 906$. *Mallet, 1880,* used two finely crystallized samples A and B, of which he deems A the best chemically. Ignition leaves $Al_2 O_3 = 102$.

Hence, atomic ratio 0.11 258, with 20 high for 27.1.

Mean of five determinations lot A was 14 high with a range of 12, all high (from 8 to 20).

The mean would correspond to 27.07.

Aluminum Sulphate, $Al_2 (O_4 S)_3 = 342$. Ignition leaves the oxide, hence atomic ratio 0.29 825. Change 41 high.

Baubigny, 1883, made two determinations giving 16 high and 2 low; mean 7 high, corresponds to 27.017 or say 27.02.

This is absolutely nearer, than Mallet's result, and on both sides of the true atomic value. By the way, Berzelius found 27.2 by this method.

But the chemical condition of the sulphate is rather improper—not crystalized.

While there is no doubt but 27 is the true atomic weight, new determinations are urgently called for, provided good methods are used.

Other Determinations have been made in considerable number. The most pretentious are those by Mallet, Phil. Transact, 1880, p. 1003.

Most of these determinations of Mallet are made according to Stasian methods, hence useless; the only exception is the case above used, but that was very much neglected by him. The use of hydrogen is out of the question, since other Stasians have thoroughly discredited it.

Dumas, in 1858, used the chloride, and Mallet, in 1880, the bromide; both determined by silver.

Such methods are useless for a metal like aluminum. They sin too greatly against the first part of the old rule of Berzelius; for they depend mainly on the "skill" of the chemist, not on the fixed conditions of the substance or the process, not on nature. See p. 3.

Chemical acrobats are no longer in demand. True At. Weights, p. 135.

$As = 75$. ARSENIC. EDGAR F. SMITH.

The most reliable work on the atomic weight of arsenic has been done at the suggestion and under the direction of Professor Edgar F. Smith in the John Harrison Laboratory of the University of Pensylvania.

The work itself has been executed by J. G. Hibbs in that laboratory; but we all know that the method and direction is the main thing.

Pyroarsenate moderately heated in a current of dry muriatic acid gas leaves a residue of salt. Hence the atomic ratio is

$4\,Na\,Cl : Na_4\,O_7\,As_2 = 234 : 354 = 0.66\ 102$. Change 38 low.

I have made a number of calculations to obtain certain checks deemed very necessary before placing this name at the head of so important an element. I think I can be reasonably satisfied. The analytical ratios here given have been calculated by myself.

Hibbs' Direct Weighings in Milligrammes.

No.	Salt.	Pyroarsenate.	Analyt. Ratio.	Excess.
1	14.39	21.76	0.66 131	29 high.
2	31.14	47.11	101	1 low.
3	38.28	57.92	091	11 low.
4	269.70	407.80	135	33 high.
5	333.28	504.40	075	27 low.
6	512.22	774.97	096	6 low.
7	547.62	828.53	096	6 low.
8	786.90	1190.68	088	14 low.
9	1106.81	1674.64	092	10 low.
10	2131.68	3224.85	102	0 low.

Not reduced to vacuum.

Below half a gramme of pyroarsenate the results are necessarily of a low apparent precision individually; but collectively, they give the mean 0.66 105 which is only 3 high. This corresponds to considerably less than one-tenth of the fall for an increase of 0.1 in the atomic weight of arsenic.

Accordingly, *these five determinations*, in which only from 20 to 500 milligrammes of pyroarsenate were used, give a mean value 75.01 for As, the individual determinations falling almost equally to both sides of this mean.

The next *four determinations* Nos. 6–9, show a systematic error, with a minimum at No. 8. The mean of these four values is 093, or 9 low. This corresponds to one-fourth of the tenth, or to 0.025 high, that is As = 75.025. The amount of pyroarsenate used is from ½ to $\frac{3}{2}$ grammes, averaging a little over one gramme.

Finally, we have *the last determination*, No. 10, in which over 3 grammes of pyroarsenate were used. The analytical ratio is exactly equal to our atomic ratio; hence As = 75, *exactly*.

Suppose we stop right here, and ask, what is the atomic weight of arsenic resulting from this series of 10 determinations, in the three groups: Nos. 1–5, 6–9, 10.

Is it not plain as day-light, that this atomic weight is 75 exactly, to which *all* determinations approach as near as possible?

Could anything more be demanded than such a close approximation? Does not such a gradually increasing series give a fine chance for the study of the work, even though we cannot always expect a perfect trajectory of errors (see True At. Weights, 1894, p. 160).

I look upon this experimental work due to friend Edgar F. Smith as the best work in atomic weight determinations produced in America. See my General Chemistry, 1897, page 378.

Lost in the Widerness of Error.

Of course, my friend Edgar, has, like many others, bowed to authority, to the great Chief Chemist at Washington, and the real Chief Center of the *American* Chemical Society. Having bowed down and competed for minute "probable errors" with the consequent "high weight" in the hands of the Chief Chemist, and a place in the Smithsonian Constants, he has forsaken the God of Truth and committed abominations. Clarke, p. 263.

Under these circumstances, Edgar F. Smith was compelled to use the atomic weights of the Chief Chemist, and thus *he falsified his own good chemical work by the use of the false auxiliary values of Clarke.*

"Ye cannot serve both God and Mammon," it was said in that old book which remains true to-day in human life and even in science.

Therefore, the atomic weight of arsenic is not 74.9158 with a probable error of 0.00222, as J. G. Hibbs in his thesis (p. 22) of 1896, reports to his Professor, Edgar F. Smith.

This value is false, resulting from the reduction of *good* laboratory work by the *false* atomic weights of the Smithsonian Institution, made by Clarke and blindly "adopted" by the American Chemical Society.

I have taken up the laboratory work done under the direction of Edgar F. Smith, and *as I do not accept human or official authority, but exclusively depend on Nature,* I have thrown out the errors introduced by the servile use of the "official" atomic weights of Clarke and the Smithsonian Institution.

As a result I find the atomic weight of arsenic to be 75 exactly, based upon the actual determinations made.

Thus, Professor Edgar F. Smith, is compelled to testify, by his work, against the official fraud and against the American Chemical Society, of which he has been President.

I consider this case a most important and highly instructive one.

If the teachings of this case are lost upon American chemists who desire to do atomic weight work, the contamination by the rotten science of Washington, has penetrated deeper than I would suppose possible.

No Reduction to Vacuum.

For my work I have taken, as stated, the direct results of the actual weighing, without the so-called reduction to vacuum. I really did not wish to enter upon explanations, because I desired to avoid all criticism not absolutely necessary.

But I know that Clarke and official science at Washington, in and outside of the Smithsonian Institution, will immediately condemn my results for omitting this reduction.

I seem to hear that penetrating voice, officially commanding:

"Reduce to Emptiness! Without Emptiness and Vacuity, "there is no Exact Science, neither in the Smithsonian "Institution, nor in the Government Bureaus.

"Reduce to Vacuum. In Vacuo Veritas."

Accordingly, I must take up this subject also; for I want this value, As = 75 exactly, to stand, because it is true.

The thesis referred to (p. 22) and Clarke (l. c., p. 215), give values reduced to vacuum, and base the atomic weight of arsenic thereupon.

By comparing these values with ours (the direct weighings in air), it will be seen that the analytical ratios reduced to vacuum are in

No.	1	2	3	4	5	6	7	8	9	10
	31	7	0	7	17	1	1	4	1	1
	low	low	low	low	high	low	low	high	low	low

compared to our direct ratios.

This shows that for the first five determinations, in which the smallest amount of substance was used (less than half a gramme), these reductions are the largest and the most fluctuating, showing an absolute range of 48 units in the fifth place.

The corresponding fluctuation in the resulting atomic weight is *four-thirds of a unit.*

For the larger amount of substance (exceeding half a gramme) the reduction to vacuum is the least and fluctuates least, the total range being only 5 units in the fifth decimal.

This corresponds to only about 0.013 on the atomic weight of arsenic.

We notice plainly the enormous "high" in No. 5 and the next in No. 8, and particularly the excessive low in No. 1.

These results show that in the new Chemical Laboratory of the University of Pennsylvania, the floor has already begun to give way very much, as it did in the laboratory of Stas, at Brussels.

If the Stasian Errors are not definitely removed from this new American Laboratory, Professor Edgar F. Smith may next time find himself several thousand yards down in the earth—as did Stas in his No. 6.

And if Professor Smith continues to use the false Smithsonian Atomic Weights of Clarke, in his American Laboratory, I shall have to leave him in that hole.

I hope that Professor Edgar F. Smith also will discontinue this humbug of reduction to vacuum. See p. 175.

The cases above specified constitute a complete confirmation of the old opinion of Berzelius concerning this reduction.

Properly made, the reduction would average about two units low in the fifth place. That is the "gnat," in the language of Berzelius.

As a matter of fact, I notice the calf of a camel in No. 8, a good sized camel in No. 5, and a big camel in No. 1, always in the language of Berzelius, borrowed by him from Matthew XXIII.

The determination made by Berzelius is often questioned by recent chemists—who ought to know better.

Thus J. G. Hibbs, in his Thesis (1896, p. 21) criticises the Old Master Berzelius, and says he found 74.840. As a matter or fact, Berzelius found 74.95 which is much nearer the truth than the value given by Mr. Hibbs, 74.9158 with the most improbable error of 0.0022 (pardon dropping the fifth, since the third even is false).

We have finally an inexpressibly funny oxidation method by means of potassium chlorate and another by means of potassium bichromate, both titrations, in the wettest of wet ways, by Kessler, 1861. The *first* gave for twelve determinations the mean *74 low*, the *second*, one series, six determinations, mean *55 high*, another series, five determinations, 46 high.

The range of the means is, therefore, 129 "only."

This is really too much for me. I cannot put that into equation, with high and low in the fifth place. I will have to put it, frei nach Heine,* melody by Stigelli:

Du hast die Chloratischen Aetzen,
Hast Alles für Saurstoff begehr,
Du hast ja Bichromat Buretten,
Mein Arsen, was willst Du noch mehr?

---

* Du hast Diamanten und Perlen
Hast Alles was Menschenbegehr,
Du hast ja die schönsten Augen
Mein Liebchen, was willst Du noch mehr?

Au = 197. GOLD.

Potassium Bromo-Aurate, $Ka\,Br_4\,Au = 556$.

The compound was reduced, and the gold filtered off and weighed; this is objectionable.

The corresponding atomic ratio is

Au : Ka Br-Aurate = 197 : 556 = 0.35 432. Change 11 high.

Krüss, 1886, made nine determinations, the best extant for gold.

Reduction was effected by SO_2 in Nos. 1, 2, 5, 6, 7; by H (dry way) in 3, 4, 8, 9. The mean of the first is 30 high, of the latter, 28 high; hence, no great difference.

The mean of all nine determinations is 29 high, corresponding to 0.26 on the atomic weight.

But we obtain quite valuable indications by collecting the results according to the amount of substance used, which substance being dried over phosphoric oxide without heat, was the most unobjectionable of all employed up to date.

Aurate, ab't	10.5 gr.	3 Det., 0.35	474	42 high.
	7.3 "	3 Det.,	457	25 high.
	5.4 "	3 Det.,	453	21 high.
Extremes:	7.0 gr.	Min. ratio,	440	8 high.
	10.6 "	Max. ratio,	476	44 high.

Total range: 36.

Here we notice a gradual approach to the atomic ratio as the amount of substance diminishes; the lowest individual result is obtained with a medium amount of substance, while the highest was obtained by the second largest amount used.

It is this very marked effect of the amount, causing a gradual approach to the atomic ratio as the amount of substance is reduced, which gives this work of Krüss its value. It plainly points the way, how to proceed in a really serious redetermination. It is, de facto, in accord with my limit method.

Since 0.1 gives the ratio 11 high, we have for

substance,	10.5	7.3	5.4 grammes.
Au =	197.38	197.23	197.19

The two extreme ratios give:

Max. 197.40. Min. 197.07.

This shows plainly that a serious gradual approach to the truth was made with the diminution of the amount of substance used.

On the whole, the more rational dry way reduction by hydrogen gas gives a mean 2 lower, and deserves the preference.

A weight of about five grammes of aurate seems most advisable.

A little further examination of the results of Krüss, stated on p. 103 of Clarke, shows that the "loss" supposed to be Br_3 was *low*, 37 and 31, mean 34 in Nos. 3, 4; and 9 and 18, mean 14, in Nos. 8, 9. The atomic ratio being 0.44 171.

This points to an incomplete reaction by the H, or to some lack of the formulated constitution in the compound. Nos. 8, 9 show smallest error.

The aggregate of *loss* with Au and Br_3 in comparison with substance taken, show a *gain* in Nos. 8 and 9 of resp. 6.79 and 2.64 mgr.

In No. 3 there was a loss of 1.28, in No. 4 a gain of 0.56 mgr.

Here we are evidently touching a very weak spot in this work.

Taking finally the analytical ratio Ka Br : Au of the residue, and comparing the same to the atomic ratio Ka Br : Au = 119 : 197 = 0.60 406, we find

Nos.	3	4	8	9
	405	365	391	398
which is	1 low	41 low	15 low	8 low

The last two, before recognized as the best determinations, give the mean 11.5 low.

The following are the conclusions that can be drawn from this entire discussion:

I. While not quite satisfactory in results, this method is good.

II. Needs careful repetition, with moderate amount (say 5 gr.), reducing by hydrogen, and

III. Checking results by ratio Ka Br : Au in residue.

Other Determinations.

Thorpe and Laurie, 1887, made, independently of Krüss, a research partly overlapping. They subject the same compound to reduction by direct heat (ignition), which in itself is better; the residue was weighed, and gold determined by washing out the bromide, and weighing the residue. They thus give up the original compound, on account of their impossibility of drying it completely without beginning decomposition, a difficulty apparently overcome by Krüss.

The low Br_3 in Krüss points to some loss in this direction. But surely, Thorpe and Laurie obtained very inaccurate results from the residue. Instead of 0.60 406 they found the mean 75 low; in this case, the Ka Br was determined by difference only.

The silver work of these analysts needs, of course, no attention here.

The determinations of Mallet, 1889, have been fully exhibited in an earlier section of this work. See pp. 24-28.

Clarke Condemns His Own Work.

In this connection we must quote the following from page 100 of Clarke's Constants of 1897 (we insert letters for reference):

"The former agreement between the several series of "gold values (a) was therefore only apparent (b), and we "are now able to see (c) that concordance among determinations may be only coincidence (d) and no proof of "accuracy (e)."

Our references:

(a) We did see no such agreement; see diagram, Plate I. Clarke's formal statement of fact, is false.

(b) Was not apparent, was none there at all.

(c) Cannot see concordance of several series or their individual determinations yet.

(d) Everybody knew that long ago; but we must still declare the absence of "concordance" in the results of Mallet, as given by himself and represented on our diagram to an exact scale.

(e) This is the kernel of the whole matter; Clarke condemns himself most unequivocally.

This is the absolute condemnation of the entire Clarke system of combining all observations by estimating accuracy (weight of observations) by minuteness of probable errors; see p. 18 and many other places.

This whole system being, in the above words of Clarke, known to himself as false and without reason, before that book of his was published at the expense of the Smithsonian Fund for the increase and diffusion or knowledge "per orbem"—every intelligent citizen of this country must ask the Chief Chemist Clarke this question:

"If you knew your entire system was false in principle, " how did you dare issue it and disgrace American Science " before the world?"

" Did you suppose your control of the American Chem- " ical Society would allow you to 'bluff' all efforts possible " of any chemist daring to expose your fraud?"

$Ba = 137$. BARIUM.

Barium Sulphate, $Ba\,O_4\,S = 233$ and *Barium Chloride,* $Ba\,Cl_2 = 208$, the latter obtained by careful heating of the crystallized compound, are probably the most stable and fixed compounds of barium. The next in order is *Barium Nitrate,* $Ba\,(O_3\,N)_2 = 261$, which crystallizes beautifully, without water of crystallization. Lastly we have the *crystallized chloride,* $Ba\,Cl_2 + 2\,H_2\,O = 244$, also a fine, very permanent compound.

These four compounds have been used by eminent chemists, from Berzelius and Turner to Struve and Marignac, for *dry way* work in atomic weight determinations.

The results are not close, and new determinations, carefully made, are very desirable.

The reactions are not the best dry way processes, either; but the methods of procedure may be improved.

The following is the record of work done:

$$Ba\,O_4\,S : Ba\,Cl_2 = 233 : 208 = 1.12\ 019.$$

Turner, 1829, 170 high; *Berzelius,* 156 high.
Struve, 1851, 2 Det. 091 — 096. Mean 75 high.
Marignac, 1858, 3 Det.; 032 — 995; 37. " 8 low.

This represents a very gradual approach to the atomic ratio, and in the last case the results fall on both sides of this value.

$Ba(O_3N)_2 : BaO_4S = 261 : 233 = 1.12\ 017.$

Turner, 1833, 3 Det.; which were 41 high, 16 high and 29 low, the mean of which is 9 high.

Now, since the mean of Marignac above was 8 low, this mean of Turner just about cancels it.

The two processes are essentially alike, the final product being the same, and curiously enough, the atomic ratio being practically the same number.

This gives rise to a peculiar check of which we may have something more to say in a near future.

$2H_2O : BaCl_2, 2H_2O = 36 : 244 = 0.14\ 754.$

Marignac, 1858, operated on 2 different samples, A and B, giving

A, 3 Det., 800 — 790; 10. Mean 41 high.
B, 3 Det., 810 — 800; 10. " 49 high.

Since now Ba = 137.1 gives the atomic ratio 6 low, this would point to Ba *below* 137, say 136.2. The dehydration, therefore, is of little value, except as to indicate that the atomic weight is *not above* 137.

The first two ratios are also rather dull, a change to 137.1 lowering the ratio about 6 only. As the analytical excesses were individually on both sides, and for the two processes about equal, representing 0.15 above and below 137, we must conclude that this value 137 is proved by the dry way processes here enumerated.

If Ba = 137.5, all these analytical excesses would be increased considerably, and the results would *all be very low.*

The wet way Silver Process has been applied repeatedly to the Chloride, and by Richards to the Bromide also. The atomic ratios are:

$BaCl_2 : Ag_2 = 208 : 216 = 0.96\ 296.$
$BaBr_2 : Ag_2 = 297 : 216 = 1.37\ 500.$

Either of these gives for 137.1 a rise of 46.

For the chloride, Marignac, in 1848, found the mean of 11 determinations, 64 high. Dumas, 1860, 16 determinations, mean 20 high. Richards, 1893, 14 determinations, mean 224 high.

For the bromide, the mean of 15 determinations of Richards was 245 high.

According to Richards' determinations, we ought to put Ba = 137.5.

We have seen that this conflicts with the dry way work, also with Dumas and Marignac, who would require only 137.05 and 137.15, or a mean 137.1.

As the case stands, we must conclude Ba = 137, until positive dry way work, in which definite compounds are weighed, proves that the dry way work done by Turner, Struve and Marignac was very badly done.

Be = 9. BERYLLIUM. NILSON, 1880.

The crystallized sulphate, $Be\,O_4\,S, 4\,H_2\,O = 177$ yields upon ignition the oxide $Be\,O = 25$. The atomic ratio is 0.14 124, rise 51 for 0.1.

Nilson and Pettersson, 1880, made 4 determinations, from 52 to 36 high; mean 45 high.

Krüss and Mohradt, 1891, by the same method, made 16 determinations, running from 37 to 10 high; mean 20 high.

The last mean corresponds to 9.04.

Bi = 208. BISMUTH. SCHNEIDER, 1851.

$Bi_2 : Bi_2\,O_3 = 416 : 464 = 0.89\ 655.$ Chg. 5 low.
Schneider, 1851, 8 Det., 682 — 634; 48. Mean 0 low.
Marignac, 1883, 6 Det., 696 — 658; 38. " 27 high.
Löwe, 2 Det., 656 — 640; 16. " 7 low.
Schneider, 1894, 6 Det., 662 — 648; 14. " 2 high.
$Bi_2\,(O_4\,S)_3 : Bi_2\,O_3 = 704 : 464 = 1.51\ 724.$ Chg. 20 low.
Marignac, 1893, 6 Det., 775 — 682; 93. Mean 4 high.

This record is sufficient. Schneider's work, of 1851, determined the value; Marignac by his new method confirmed it; Schneider, in 1894, settled the question. We have no room for rubbish.

Bo = 11. BORON. RAMSAY, 1893.

We have given all necessary data and methods very fully in Part III, pp. 141–159.

We may add the determination by water of crystallization of borax on account of its historical interest (compare pp. 42-44) Borax = $Na_2 O_7 Bo_4 + 10 H_2 O$.

Water: Borax = 180 : 382 = 0.47 120.

Berzelius, 1826,	3 Det.	Mean 10 low.
Laurent, 1849,	2 Det.	" 55 high.
Hoskyns-Abrahall, 1892:	5 Det., 230 high; 87 high.	" 167 high.
Ramsay and Aston, 1893:	7 Det., 83 high; 10 low.	" 48 high.

Br = 80. BROMINE. MARIGNAC, 1843.

Br : Ag = 80 : 108 = 0.74 074. Change 93 high.

Marignac, 1843,	3 Det., 072 — 055; 17.	Mean 3 high.
Stas, 1865,	1 Synthesis.	" 9 low.
Stas, 1865,	4 Det., 083 — 079; 4.	" 7 high.
Wet Way Titrations:		
Huntington, 1881,	6 Det., 111 — 035; 76.	" 3 low.
Richards, 1890,	6 Det., 076 — 044; 32.	" 9 low.
Richards, 1893,	11 Det., 089 — 034; 55.	" 7 low.

These three series were made in connection with determination of Cd, Cu, Ba.

Other, perfectly concordant determinations by Cooke (Sb), Thorpe (Ti), Thorpe and Laurie (Au) can only be mentioned.

It will be noticed, that Marignac-Huntington, and Stas-Richards' (2) exactly balance.

The atomic weight of bromine has long been known to be exactly commensurable to that of silver, namely, Br : Ag = 80 : 108 = 20 : 27.

All other work done is either not direct or has been properly placed in the Chemische Rumpelkammer.

We have no space for either here.

C = 12. CARBON-DIAMOND. DUMAS, 1840.

All necessary data given; see p. 39 and pp. 101–105.

Ca = 40. CALCIUM.

All necessary data given; see pp. 106–108.

The work is not as concordant as required for so important an element.

Critical research on the methods used have been carried on for some years and will be completed by actual final determinations as soon as time shall permit.

Cd = 112. CADMIUM. v. HAUER, 1857.

$CdS : CdO_4S$ = 144 : 208 = 0.69 231. Chg. 15 high.
1 Karl v. Hauer, 1857, 9 Det., 257 — 209; 48. Mean 0 high.
2 Partridge, 1890, 10 Det., 205 — 185; 20. " 32 low.
$Cd : CdO$ = 112 : 128 = 0.87 500. Chg. 10 high.
3 Morse and Jones, 1892:
10 Det., 508 — 504; 4. Mean 7 high.
4 Lorimer and Smith:
9 Det., 518 — 491; 27. " 4 high.
5 Bucher, 1894, 2 Det., 511 — 504; 7. " 8 high.
6 Bucher, 1894, 3 Det., 491 — 484; 7. " 13 low.
7 Morse and Arbuckle, 1900, Absorption of Oxygen?
3 Wet Way. 4 Electrolysis. 5 Porcelain Crucibles, both.
6 Pt Cr. in Porcelain Crucible.
$CdO : CdO_4C_2$ = 128 : 200 = 064 000. Chg. 18 high.
8 Lenssen, 1860, 3 Det., 053 — 982; 71. Mean 10 high.
9 Partridge, 1890, 10 Det., 971 — 957; 14. " 36 low.
10 Morse and Jones, 1892:
5 Det., 008 — 996; 12. " 3 high.
14 Bucher, 1895, 8 Det., 014 — 951; 63. " 22 low.

M. and J. finding subst. slightly hygroscopic, took necessary precautions; hence, probably "9" higher.

$CdS : CdO_4C_2$ = 144 : 200 = 0.72 000. Chg. 14 high.
12 Partridge, 1890, 10 Det., 979 — 968; 11. Mean 27 low.
13 Bucher, 1895, 10 Det., 065 — 037; 28. " 51 high.

No. 1, Standard; No. 2, some error; Nos. 3, 6, reasonable, give corresponding atomic weight below 112.1. Oxalate-Work, Nos. 8 to 13, inferior, as might be expected.

Electrolysis, Hardin, 1896:

14 Cd : Cd Cl_2, 10 Det., 252 — 236; 16. Mean 42 high.
15 Cd : Cd Br_2, 10 Det., 210 — 196; 14. " 27 high.

By Silver:

16 Cd Cl_2, Dumas, 1860:
6 Det., 083 — 618; 465. " 121 high.
17 Cd Br_2, Huntington, 1881:
8 Det., 110 — 045; 65. " 150 high.

By Silver:

18 Cd Cl_2, Bucher, 1895:
21 Det., 949 — 880; 69. " 153 high.
19 Cd Br_2, Huntington, 1881:
8 Det., 437 — 405; 32. " 82 high.
20 Cd Br_2, Bucher, 1895:
5 Det., 480 — 453; 27. " 124 high.

These results speak for themselves. Even electrolysis not satisfactory; but both silver processes worthless.

Ce = 140? CERIUM.

The following two reactions have been preferred by chemists:

$2\ CeO_2 : Ce_2(O_4S)_3$ and $2\ CeO_2 : Ce_2(C_2O_4)_3$

The results are still unsatisfactory.

Ratio:	A, Sulphate.	B, Oxalate.
Ce = 139	0.60 429	0.63 100
140	563	235
141	702	370
Difference per unit	139	135

A. Brauner, 1885, 23 Det., 604 — 549; 55. Mean 0.60 573.
B. Buehrig, 1875, 5 Det., 0.63 432.

Possibly Ce = 140.

Cl — 35.5. CHLORINE. TURNER, 1833.

See pp. 97–101.

The various Stasian Reactions have been disposed of in the preceding, and are fully reviewed in my True Atomic Weights; they have finally been thrown into the Rumpel Kammer.

Co = 59? COBALT.

$Co : CoO = 59 : 75 = 0.78\,667$. Change 28 high.
1 Russell, 1863, 14 Det., 614 — 550; 64. Mean 75 low.
2 Zimmermann, 1886, 10 Det., 638 — 630; 8. " 32 low.
3 Krüss,
4 Remmler, 1891, 24 Det., 859 — 508; 351. " 54 low.
$2\,Co : Co_2Cl_6, 10\,NH_3 = 118 : 501 = 0.23\,553$. Chg. 30 high.
5 Sommaruga, 1866, 7 Det., 858 — 806; 52. Mean 274 high.
6 Lee & W. Gibbs, 1871, 6 Det., 587 — 569; 18. " 27 high.

In 4 gradual fractioning, Oxide points to 0.1 low; but purpureo-cobalt chloride points to as much high. If both substances equal in value, Co = 59.

Cr = 52. CHROMIUM.

$Cr_2O_3 : Am_2O_7Cr_2 = 152 : 252 = 0.60\,318$. Chg. 31 high.
1 Rawson, 6 Det., 368 — 330; 38. Mean 28 high.
2 Meinecke, 1891, 5 Det., 353 — 320; 33. " 14 high.
$Cr_2O_3 : Ag_2O_7Cr_2 = 152 : 432 = 0.35\,185$. Chg. 30 high.
3 Berlin, 1846, 1 Det., 121 high.
4 Sievert, 1861, 2 Det., 262—139; 123. Mean 16 high.
$Cr_2O_3 : 2\,AgO_4Cr = 152 : 664 = 0.22\,892$. Chg. 23 high.
5 Berlin, 1846, 4 Det., Mean 122 high.
6 Sievert, 1861, Shows why high.
7 Meinecke, 1891, 6 Det., 943 — 924; 19. Mean 39 high.

These are the only determinations worth consideration; the final product is the sesquioxide, obtained by ignition. They suffice to fix the unit.

Cs = 133. CESIUM.

$CsCl : AgCl = 168.5 : 143.5 = 1.17\,421$. Chg. 70 high.
1 Johnson & Allen, 1863, 4 Det., 580—399; 181. Mean 78 high.
2 Bunsen, 1863, 3 Det., 503—435; 68. " 46 high.
3 Godefroy, 1876, 4 Det., 265—107; 158. " 257 low.
Bunsen's mean corresponds to 133.06.

But since only the silver chloride process has been employed, this result is not final.

Cu = 63.5 COPPER.

Blue Vitriol = $CuO_4S, 5H_2O$ = 1.00 000

gives atomic ratios:	CuO_4S = 0.63 928
Loss: H_2O = 0.36 072	CuO = 0.31 864
" $H_2O + SO_3$ = 0.68 136	Cu = 0.25 451

1 Richards, 1891, Cu by electrolysis. 4 low, 1 high, 3 high.
H_2O heat 5 high, 5 high.
Detail, True Atomic Weights, 1894; p. 132.
$Cu : CuO$ = 63.5 : 79.5 = 0.79 874. Chg. 25 high.
2 Erdmann and Marchand, 1844:
3 Det., 878 — 860; 18. Mean 2 low.
3 Millon and Commaille, 1863:
3 Det., 791 — 770; 21. " 95 low.
4 Hampe, 1873, 3 Det., 838 — 831; 7. " 39 low.
5 Richards, 1891, 2 Det., 820 — 802; 18. " 63 low.

Richards (5) is in conflict with (1); but Erdmann and Marchand (2) are in concord.

It is of course, understood that Professor Richards finds quite another value for copper, and how he succeeds in doing so we have shown in our True Atomic Weights, 1894, pp. 128–136; also, before that time, in Zeitschrift f. Anorg. Chemie, Bd. v., pp. 293–298; 1893, and in Chemical News, Oct. 6, 1893.

It is, as in the case of Crookes: The work is there, but tangled up with errors of all kinds; we have unraveled the tangle and showed the analysts what the results of their work really is.

Fe = 56. IRON. SVANBERG, 1844.

All necessary data given, pp. 91–95.

Fl = 19. FLUORINE. LOUYET, 1849.

$CaO_4S : CaFl_2$ = 136 : 78 = 1.74 359. Chg. 446 low.
Record to 4 places only: 1.74 36. " 45 low.
Berzelius, 1826, Fluorite, 3 Det., 1.750. " 64 low.

Louyet, 1849,	Fluorite,	6 Det.	Mean 1.7437,	1 high.
	Artif.	3 Det.	" 1.7417,	19 low.
De Luca, 1860,	Fluorite,	4 Det.	" 1.7459,	23 high.
Dumas, 1860,	Fluorite,	1 Det.	" 1.7455,	19 high.
Moissan, 1890,		2 Det.	No data given.	
$Na_2 O_4 S : 2 Na Fl = 142 : 84 = 1.69\ 048$.				Chg. 402 low.
Record to 4 places only:			1.69 05.	" 40 low.
Louyet, 1849,		3 Det.	Mean 1.6847,	58 low.
Dumas, 1860,		2 Det.	" 1.688,	25 low.
Moissan, 1890,		5 Det.	No data given.	
$Ka_2 O_4 S : 2 Ka Fl = 174 : 116 = 1.50\ 000$.				Chg. 258 low.
Record to 4 places only:			1.5000.	" 26 low.
Dumas, 1860,		2 Det.	Mean 1.4991,	9 low.
$Ba O_4 S : Ba Fl_2 = 233 : 175 = 1.33\ 143$.				Chg. 152 low.
Record to 4 places only:			1.33 14.	Chg. 15 low.
Louyet, 1849,		3 Det.	Mean 1.331,	4 low.
Moissan, 1891,		2 Det.	No data given.	

It is to be regretted that the determinations of Moissan cannot be used, he having failed to give any direct weighings. See p. 35.

The principal data are those of Louyet, who lost his life on account of this work. It is sufficient to establish $Fl = 19$. On account of the low value, four decimals are perfectly sufficient.

By the value given for the change, the analytical excess stated, give for Louyet's determinations, from Fluorspar 18.999, from the sodium salt, 19.15 and from the barium compound, 19.025. As the pure fluorite is probably the purest, the standard 19 is the true, absolute value within the limit of precision, here at most 0.01.

$H = 1$ HYDROGEN.

This atomic weight is commonly taken as unit. But it cannot be taken as standard, for hydrogen cannot be weighed with precision, being a gas.

We have said sufficient on the questions here involved, especially in our True Atomic Weights. It will incidentally be shown here also.

The same is true to the exact value of this atomic weight in reference to that of oxygen; see True Atomic Weights, pp. 177–183 on the determinations of Dumas, and pp. 185–189 on determinations by Keiser, Morley, Ditmar and Julius Thomsen.

Lord Rayleigh's Determinations.

The most important work on hydrogen we have incidentally given on page 162, in the density determinations by Lord Rayleigh, namely, $H = 1.0028$ for $O = 16$ exactly.

An earlier determination by the same authority is 1.0062; see p. 161. This was in 1893, before the discovery of argon. The above, much smaller value was obtained *after* this discovery, which incidentally increased the experience in this work very greatly.

We, therefore, may conclude $H = 1.0028$ to represent an *upper limit* for hydrogen.

This, the most difficultly purified, most difficultly retained pure gas, shows, in the hand of this master, a density-atomic weight of 1.006 in 1893, and only 1.003 in 1897.

We conclude that the true atomic weight of absolutely pure hydrogen is 1.000, to which Lord Rayleigh approached within 0.006 in 1893, and to within half that limit (0.003) in 1897.

Every impurity necessarily raises the density of this, the lightest of all gases.

Morley's Determinations.

The most pretentious work of *Morley* on hydrogen, has appeared since the publication of our work of 1894. It was "recommended for publication by F. W. Clarke," and published in grand style by the Smithsonian Institution as No. 980 of its "Contributions to Knowledge," in 1895.

I do not deem it necessary to enter upon the "complete syntheses" of water by Morley; for this element, it is preferable to use density determinations, as for nitrogen. We have not time to look into the many more numerous errors possible in that synthesis.

Edward W. Morley is a thorough Stasian—with all that implies; he glories in it. We can take space only for a very few particulars.

Being also very Clarkian, he aims above all at minute "probable errors;" that is, de facto, close concordance. He shapes his work with a special view of this requirement.

This is desirable, within certain restrictions; *but it is the height of folly to suppose that mere concordance is truth.* We have shown that repeatedly.

Morley's Show of Precision.

Morley makes a wonderful show *of precision. This often is fatal.* For example, he gives full record of weight comparisons (p. 31) for 1887 and 1892; to *eight decimals* for the gramme set, to five decimals for the milligramme set.

He does not give *deviations* merely; that would look too simple and would be practical. No, he gives complete numbers.

Thus, his 10-gr. piece, in 1887, was 38, in 1892, only 8 hundredths *mgr.* light.

He puts this 9 999.62 and 9 999.92 which looks impressively exact to the common chemical public, but would be too ridiculously cumbrous for actual use.

It is, of course, curious to note that this ten-gramme piece gained in weight by use during five years.

The 100-gr. piece was short 66 and 49 hundredths mgr. in 1887 and 1892, but the 50-gr. 3 short in 1887, and 36 long in 1892.

Wonderful precision—suspiciously so, when stated that the weights have been little used and handled with great care.

It might have proved interesting to have such "exact" determinations made at shorter intervals. The changes are too funny.

But we have room only for the results of his weighings of hydrogen and oxygen, expressed in grammes per liter (his density).

Weighings of Oxygen.

For *oxygen* he made three series of weighings according to as many methods. He gives his results to 5 places. We will leave them at that; but add the extremes and range to the means.

		Mean.	Extremes.		Remarks.
I.	9 Det.,	1.42 879	907 — 838;	69.	
II.	9 Det.,	890	952 — 853;	99.	"globe 5"
	6 Det.,	869	880 — 851;	29.	"globe 3"
	All 15,	1.42 887			
III.	7 Det.,	918	957 — 860;	97.	from chlorate.
	10 Det.,	908	951 — 849;	102.	" electrolys.
	All 17,	1.42 917			

Morley gives the probable error of these three means to the *millionth* of the gramme, of course. We will give them to the fifth decimal; they are claimed to be 3, 5, 5 in the fifth place, that is, the last decimal given above.

But even if these were the probable errors of the means, they would invalidate the fifth decimal.

As a matter of fact, we see the *third* decimal changes! But let us continue to talk in the *fifth*, as above printed.

We find that the mean for "globe 3" is 21 less than for "globe 5." But 21 in fifth place amounts to $\frac{1}{5}$ or *two-tenths of a milligramme!*

To what is this change due? To anything about the gas? Oh, no!

The gas weighed is the same; but it does not weigh the same, because it is contained in a different glass globe!

But don't Morley allow for the weight of his glass globes? Surely, he does, with the utmost precision, and gives the full data on pp. 29 and 30.

Why then this difference? Well, that is just the trouble with our Stasian exact chemists, such as Morley.

They are so exact—oh, it is absolutely wonderful *to read!*

And when they get through, some strange error stares them in the face, for which they cannot account.

They may strain at the gnats—but the camel they cannot hide, although they—don't mention him.

In fact, the above little " camel " *whom* we found roaming about in the wilderness of Morley's figures, is not mentioned by Morley.

Yet, on results, claimed exact to the fifth decimal, having on the mean only a probable error of 5 units in the fifth, we detect a difference of *21 units* in the fifth between the MEANS of six determinations made in "globe 3" and nine determinations made in "globe 5." This difference *between the means* is over four times the amount of the probable error of the mean of all 15 determinations, and amounts to *two-tenths of a milligramme.*

But two-tenths of a milligramme is a quantity of common recognition in all good chemical analysis.

Now, why does not Morley show up this great conflict of the means according as his oxygen was weighed in "globe 3" or "globe 5," the weight of which was properly and minutely allowed for?

Possibly he did not notice this little camel; he was too busy straining at gnats. At any rate, he does not say anything about it.

We notice next, that the oxygen from chlorate was 0.1 mgr. heavier per litre than the electrolytic oxygen.

We notice that the third series gave, in both of its divisions, a *full milligramme* difference between the lightest liter and the heaviest liter.

But a milligramme is a full grown camel, when the gnats, are given to the hundredth of the milligramme, and the "probable error" are gnats of the thousandth of a milligramme.

In first division of Series II, the range is also 99-100, that is, a full milligramme. In the first series it is ⅔ of a milligramme.

What must we conclude from these facts? That the work of Morley is so wonderfully exact as Clarke, and the Secretary of the Smithsonian, and the Stasian Ostwald, and the admiring members of the American Association or the American Chemical Society are expected to believe and vociferously do believe?

Is not this "*credo*" of exact science shamefully ridiculous?

We can not afford to take conclusions "ready made" by authorities at Washington or at Berlin; we have to be convinced by the *facts,* and not by the apparent forms of accuracy and the official dictum.

And seeing, that the *means* of a liter of oxygen *actually vary* to the extent of $\frac{1}{5}$ mgr. according as it is contained in one or another globe;

and seeing, that the weight of a liter of oxygen is de facto varying to a full milligramme in different determinations in three series (II first, III first and second);

we must admit these facts as the true limit of the accuracy attained,

although Mr. Morley may imagine that he has attained to the precision of his "probable error" of *one-twentieth* of the milligramme!

Morley has determined the weight of a liter of oxygen with an uncertainty of one milligramme at best in any one of these series.

But how much is this weight, to the nearest milligramme?

Why, that is rather uncertain, again. Let us see. We will give both Morley's own means, and ours rounded off at the proper place.

Weight of One Liter of Oxygen, in Grammes.

	Morley.	"Rounded off."	Determinations.
Series I	1.42 879	1.42 88	9
" II	887	89	15
" III	917	92	17
		Mean 1.42 9	41

We notice a distinct and gradual *increase* in the weight, from the first to the last series of Morley; the total change amounting to almost *half a milligramme.*

These are the actual scientific results; the "fifth" decimal is imagination, pure and simple.

Morley's Weighings of Hydrogen.

We must next consider Morley's weighings of hydrogen.

He gives his results to the millionth of the gramme per liter; that is the thousandth of the milligramme.

Wonderful, truly wonderful, is the precision of Morley's work—*on paper.*

Turning to page 63, where the 19 determinations of his second series (made according to his second method) are recorded, we find:

	Grammes.
No. 17	0.089 869
No. 18	0.090 144

These two consecutive determinations, made when Morley had gained experience in making 16 determinations preceding in this series, and all 15 determinations of the first series, differ by

0.000 275 grammes.

This is *275 millionths* of a gramme, or 0.275 of a milligramme; over a quarter of a milligramme!

But when consecutive determinations differ by almost *three tenths of a milligramme*—what is the use of giving us thousandths of the milligramme?

This is either humbug or fraud, unless it be a contemptible mixture of both; it is Stasian.

We conclude that the weighings to the thousandths of the milligramme are simply for show, and for fooling the members of the American Association and of the American Chemical Society.

We shall have room for only a complete summary of his five series, but shall express the results in milligrammes for convenience. We shall not drop any of his fine work, for the thousandth of a milligramme is just the same as the millionth of the gramme.

Morley: Weight, in mgrs., of liter of hydrogen.

Series I	15 Det.,	993 — 846;	147.	Mean	89.838
“ II	19 Det.,	144 — 869;	275.	“	970
“ III	6 Det.,	912 — 856;	056.	“	886
“ IV*	6 Det.,	972 — 777;	195.	“	880
“ V	11 Det.,	883 — 830;	053.	“	866
Mean of all	57 Det.,	144 — 777;	367.	“	89.873

The means are those actually given by Morley, in which a final little gnat of one thirty-thousandth was allowed for (see his p. 28), which amounts to 3 units in the last decimal here given, that is to

one-third of a hundredth of a milligramme!

---

* The extremes of this series are put by Morley in brackets [ ] commonly used to indicate their exclusion from consideration.

Being rather greatly divergent, our friend of concordant results naturally desires to suppress these results. But he fails to give any reason for this exclusion. Hence we must keep them, as Morley's own fine determinations.

Looking at the above tabulated means as given by Morley himself, we recognize the utter folly of such a "correction," except for *show* of an exactness NOT attained to in fact.

For we see readily, that *the longer Mr. Morley kept weighing hydrogen, the lighter it became*, precisely, as our corresponding table for oxygen showed plainly (though we did not accentuate it in words) *that oxygen grew heavier on his hands*, the longer he kept weighing it. See p. 248.

Suppose, for a moment, that Mr. Morley had not stopped weighing these two gases—would *that be any reason for the hydrogen ceasing to grow lighter?*

I should not think so; of course, Morley weighing with utmost exactness, can not, and therefore has not erred—except in ceasing to weigh altogether.

We shall resume this fascinating subject further on.

We must first obtain the mean weight of a liter of hydrogen, according to the exquisitely accurate weighings of Mr. Morley, made to the thousandth of the milligramme.

But—that is absolutely impossible, the weight is not irregularly varying, but varies gradually and steadily.

Ah, indeed! That is the trouble. Morley has got it. Of course he was liable to catch it. Indeed, he is afflicted with Morbus Stasii, very badly.

Let us drop, therefore, that humbug of the thousandths of a milligramme. We have shown it to be a humbug here—as we have in other cases.

We shall have all the uncertainty we want to touch with these, our hands, if we round off the above results of Morley to the hundredth of a milligramme.

Dropping the Thousandths of the Milligramme.

Mean weight of a liter of hydrogen in mgrs.

Series	I	15 Det.,	99 — 85; 14.	Mean	89.94
"	II	19 Det.,	14 — 87; 27.	"	97
"	III	6 Det.,	91 — 86; 5.	"	89
"	IV	6 Det.,	97 — 78; 19.	"	88
"	V	11 Det.,	88 — 83; 5.	"	87

Combining, we obtain

Series	I, II	34 Det., 14—85; 29.	Mean	89.95
“	III, IV, V	23 Det., 14—78; 36.	“	88
Mean of all,		57 Det., 14—78; 36.	“	89.91

Actual extremes are:

Highest, Series	II.	90.14 mgr.
Lowest,	“ IV.	89.78 “
Absolute Range,		0.36 “

The actual range of the weights of one liter of hydrogen is, therefore, a little over

one third of a milligramme,

which is exactly one hundred times the weight of the last gnat strained out by Morley. Page 249, supra.

Of course, such straining may strain the strongest man. It has affected Mr. Morley seriously and induced the Morbus Stasii.

But we must close this subject. We shall here give the general *mean* of all determinations, the extreme means of a single series, and the actual extremes of the entire work.

Weight of Liter in Mgr.	Hydrogen.	Oxygen.
Absolute lowest,	89.78	1428.3
Lowest mean,	89.87	1428.8
Mean of all,	89.91	1429.0
Highest mean,	89.97	1429.2
Absolute highest,	90.14	1429.6
Absolute Range,	0.36	1.3
Uncertainty, one in	250	1100

These are the actual facts. The total range actually covered does not necessarily *include* the truth. We remember, that almost all coins actually weighed were light—so also, *all weight of a liter of a given gas may be either light or heavy*, as compared to the actual range of weight, *according as there may lurk a trace of a lighter or heavier gas, in the supposedly pure gas!*

Now, we do know heavier gases that no means of purification used by Morley could have removed—such as argon. On burning the hydrogen to water, the argon would remain dissolved in the water, under the exact conditions used in Morley's complete synthesis of water.

But we have not time nor space to enter upon that subject now.

Morley's Ratio O : 16.

Let us divide the actual weight found for a liter of oxygen by 16, the standard atomic weight of oxygen; the quotient will be the weight of our liter of oxygen *per unit* of atomic weight.

This weight must be compared to the weight actually found by Morley for a liter of hydrogen.

1 Liter of:	Oxygen.	$\frac{1}{16}$ Oxygen.	Hydrogen.	Excess.
Lowest,	1428.3	89.27	89.78	0.51
Mean,	1429.0	89.31	89.91	0.60
Highest,	1429.6	89.35	90.14	0.79

All weights in milligrammes.

We see that hydrogen, according to Morley's weighing, exceeds the sixteenth part of the weight of oxygen by from 0.5 to 0.8 a milligramme per liter.

It is not constant, this excess, because the actual uncertainty in the weight of hydrogen is one in 250 and of oxygen one in 1100 only; that is, oxygen is weighed with 4 to 5 times the precision attained for hydrogen.

The data given in our table (p. 251), plainly show that the uncertainty for hydrogen is a little less than one-half of one per cent, while for oxygen it is less than one-tenth of one per cent.

Accordingly we might well accept the *sixteenth* of the weight of oxygen as a more reliable determination than the directly weighed hydrogen.

As a matter of fact, Morley left an uncertainty of 0.36 mgr or over ⅓ of one milligramme in the weight of one liter of hydrogen.

This is but a very little less than the actual difference between the weight of a liter of hydrogen and the one-sixteenth of the weight of a liter of oxygen.

Compare General Chemistry, 1897, p. 378.

Can these results of Morley be taken as positive experimental demonstration that O : H is *not* 16:1?

When Lord Rayleigh found "atmospheric" nitrogen a trifle "high" did he conclude the Laws of Nature to be false?

No; he suspected some heavier gas present as an impurity that had escaped the reagents used for purification.

He tried to isolate it, and discovered argon.

Does Morley's hydrogen contain a trace of argon? What is it that causes this slight difference in weight, almost covered by the actual uncertainties of weighing brought out by us?

Is it simply due to errors of handling and weighing of hydrogen? Let us remember that Morley found oxygen much heavier in "globe 5" than in "globe 3."

Probably that is all. At any rate no certain difference of the atomic weight by hydrogen has been established by Morley from the value of the sixteenth of that of oxygen.

Morley *says* his gases were *pure*; yet he reports having found CO_2, N, etc., in these pure gases, and that he don't know how they got there!

Morley's Ratio O:H.

We want to obtain the ratio of oxygen to hydrogen, and also the value of hydrogen for oxygen taken at 16.

According to Morley, the first is 15.879; this has been "*adopted*" by the Chief Chemist and made the basis of his Smithsonian System of Atomic Weights (Constants of Nature, 1897, p. 33).

Therefore, it is—taken as true by all official chemists of the United States and by the American Chemical Society.

But is this really the expression of Morley's determinations?

Oh, not at all, as every one familiar with the determination of the QUOTIENT of two experimental data knows.*

---

*This professional blunder is most remarkable, since nearly all common works on quantitative research give specific directions on this point.

See Kohlrausch's Leitfaden, IV ed., 1880, p. 7-9, which treats this very case of the error of a quotient, such as a specific gravity determination.

The simplest is my method in my Elements of Physics, 1870, p. 12.

The corresponding condition in Ostwald's Hand und Hilfsbuch, 1893, p. 4, last case, is *not* fulfilled, for O is over four times as accurately determined as H. See p. 252.

If we have several determinations, can we take simply the quotient of the means?

Hardly. In the present case, we found even the *means* changing during the progress of the work of Morley.

His oxygen became gradually heavier, his hydrogen gradually decidedly lighter; see pp. 248 and 250.

Accordingly, the ratio O : H has grown greater as Morley became more expert by practice.

If Morley had kept up the work, there is no telling how much this ratio would have grown.

Let us see, as to his *means*.

Earliest Series:

Oxygen, Series I 1428.79 mgr.
Hydrogen " II 89.97 "
giving the ratio $O : H = 15.881$
and for $O = 16$ exactly, $H = 1.0075$.

Last Series:

Oxygen, Series III 1429.17 mgr.
Hydrogen, " V 89.87 "
giving the ratio $O : H = 15.902$
and for $O = 16$ exactly, $H = 1.0061$.

Here we have an actual increase of the ratio from 15.88 to 15.90 and a corresponding diminution of the atomic weight of hydrogen from 1.008 to 1.006, stopping at the third place.

Now if, on the means, Morley gradually weighed oxygen so much heavier and hydrogen so much lighter, that he reduced the excess of the atomic weight from 8 to 6 thousandths, how long would he have had to keep training and improving in skill, to reduce this excess to nothing, and prove $H = 1$ exactly?

We shall not undertake to answer this question.

But we shall again turn to the actual results of Morley. This time the absolute extremes will be called for.

It is, of course, understood that we dare not touch one of these determinations of Morley. They are made by a master, all of them. Sometimes the very extremes are nearest the truth, as we have repeatedly found.

Hence, let us see what the extremes teach us

Oxygen, highest,	1429.6	mgr.
Hydrogen, lowest,	89.78	"

giving the ratio O : H = 15.923
and for O = 16 exactly, H = 1.0048.

Oxygen, lowest,	1428.3	mgr.
Hydrogen, highest,	90.14	"

giving the ratio O : H = 15.846
and for O = 16 exactly, H = 1.0098.

The actual determinations by Morley, therefore, range as follows:

For Oxygen.	Ratio O : H.	Atomic Weight H, for O = 16.
Absolute lowest,	15.846	1.0098
Mean, "	15.881	1.0075
Mean of all,	15.894	1.0067
Mean highest,	15.902	1.0061
Absolute highest,	15.923	1.0048

This is the true expression of the actual determinations made by Morley.

We may here repeat the values obtained by Lord Rayleigh; see p. 244.

Hydrogen, for O = 16, exactly.	
1893	1.0062
1897	1.0028

It seems to us that Morley has been doing reasonably well; if he only had kept up his practice, he might again have halved his smallest value and come up to the level of Lord Rayleigh, in 1897.

Within the range of Morley's determinations of the weights of a liter of oxygen and of hydrogen, he has obtained results ranging

for H, from 1.0098
to 1.0048

the excess above 1 having ranged from 98 to 48 ten-thousandths, which is the same as from ten to five-thousandths.

How much longer would he have had to practice, to drop the other half of his original excess?

At the same time, the ratio O : H *ran up* from 15.85 to 15.92 or seven-hundredths.

How much longer would Mr. Morley have had to practice to gain another such seven-hundredths, and prove the ratio 15.99 which is no doubt near the true 16.00?

Morley's Hopeful Progress.

Judging from the very notable progress Mr. Morley made in this direction, it must be regretted that he stopped practicing so soon.

But we doubt very much whether Mr. Morley would have been able to accomplish this work. His aim was simply concordance, as required by Clarke.

He never tried to find the true value; for he cannot admit the possibility of constant errors, he is too much affected by Morbus Stasii, complicated with a very natural development of Furor Clarkii.

But we have, in our examination of his results, shown up several notable cases of aggravated acerberations of Morbus Stasii.

For example, the fact that his gas was intrinsically heavier in one globe than in another, he did not notice any more than Stas became aware of the floor of his laboratory sinking four thousand yards with him when reducing his silver nitrate No. 6 to vacuum.

We have said enough about this whole investigation.

It has been the expressed ambition of Mr. Morley to emulate Stas. I am sure he has succeeded admirably.

The results of Stas will have to be hunted up in the Chemical Rumpelkammer.

The larger part of the work of Morley will be found in the same place—close by the work of Stas.

The Stasians have imitated their master and teacher in calling every chemist not accepting their doctrine, a visionary, depending upon imagination, and believing in chimeras.

We have pulled the lion skins from the animals parading as great chemists, most excellent and accurate workers, unexcelled in precision.

The first reaction, when deprived of that false wrap, has been a lusty bray: extrapolation, selection, imagination.

Then have come misrepresentation and actual fraud.

We will leave them at that stage to their own meditation.

State Science and State Church.

By reference to pp. 47–49 and p. 187 of my True Atomic Weights, of 1894, every chemist can see how courteously I treated Messrs. Clarke and Morley.

Drunk with power, and full of the false notion of experimental exactness, they have acted as brutes, at meetings in Washington, Madison, and in their publications.

I, therefore, have had no reason to state the case otherwise than regard to scientific truth requires.

It is a most deplorable fact that the General Government has gone into the building up of a Science Trust of the most formidable character, now using nearly ten million dollars a year.

STATE SCIENCE is already now showing greater corruption and demoralization than STATE CHURCH could boast of after a thousand years of power in all christendom.

When the most famous of the branches of State Science at Washington, namely, the Smithsonian Institution, publishes as true and highest science a work so unspeakably corrupt in every fiber as the so-called Constants of Nature of Clarke, with its falsehood-page 6, it is impossible to use language strong enough to condemn this system.

There is just as much reason for our Government to take in hand the building up of a National Religion, as it can find for its actual building up of a National Science at the lavish expenditure of millions of dollars a year.

Is there any pressing necessity for our Government building up an Academy of Laputa at Washington, such as Gulliver describes?

Hg = 200. MERCURY. ERDMANN, 1844.

The necessary details have been given, pp. 95–96, for the oxide, pp. 96–97 for the sulphide, and pp. 97–99 for the chloride. See also bromide and cyanide, p. 100.

In = 113.5. INDIUM. WINKLER, 1867.

$In_2 : In_2O_3 = 227 : 275 = 0.82\ 546$. Change 13 high.
Winkler, 1867, 3 Det., 581 — 537; 44. Mean 10 high.
Bunsen, 1870, 1 Det., 589. " 43 high.

The radio-tests of Benoist (C. R. 132,772; 1901), confirm it to be a sesquioxide.

The work of Winkler needs being confirmed. His two gold-substitutions

$$In : Au = 113.5 : 197 = 0.57\ 614$$

gave values rather high, namely, 208 high; but we know that this process cannot give accurate results.

Io = 127. IODINE. MARIGNAC, 1842.

Ka Io : Ag = 166 : 108 = 1.53 704. Change 92 high.
Marignac, 1842, 5 Det., 794 — 651; 143. Mean 5 low.

This corresponds to only 0.005 on Iodine.

Clarke (1897, p. 48), says this work of Marignac was "without remarkable accuracy." The range is rather high, but the mean is all right, and that is what Clarke always wants; even his own probable error of this mean is only 18 in our units; but that corresponds to 0.02 only on the atomic weight, which ought to be good enough.

Ag. Io : Ag = 235 : 108 = 2.17 593. Chg. 92 high.
Marignac, 1842, 3 Det., 519 — 500; 19. Mean 59 low.
Stas, 1865? a, 2 Det., 536 — 529; 7. " 60 low.
" b, 6 Det., 543 — 530; 13. " 56 low.
" c, 5 Det., 539 — 529; 10. " 60 low.

Since Clarke has pleased to throw his reproach upon Marignac, we here take these determinations from the Rumpel Kammer, to show, that Marignac's work was equal to that of Stas, as even Clarke has to admit (l. c., p. 49).

All these determinations are about 60 low, which represents 0.06 low on the atomic weight of iodine, as readily seen.

Ir = 193. IRIDIUM.

$Ir : Ka_2Cl_6Ir = 193 : 484 = 0.39\ 876$. *Chg. 12 high.*
Seubert, 1978, 8 Det., 890 = 868; 22. Mean 4 high.

The corresponding Ammonium Salt gave values 67 high in mean, but deserve less credit.

For 192.5, the atomic ratio is 0.39814.

Ka = 39. POTASSIUM.

$KaO_3N : KaCl = 101 : 74.5 = 1.35\ 570$. Chg. 47 low.

Penny, 1839, Ser. I,	7 Det., 640 — 630; 10.	Mean 66 high.
II,	7 Det., 641 — 628; 13.	“ 63 high.
Stas, 1865,	7 Det., 655 — 638; 17.	“ 75 high.
Hibbs, 1896,	5 Det., 642 — 620; 22.	“ 57 high.

These give in this order, 014, 0.13, 0.18 and 0.12 high. Stas gives 39.18, Hibbs 39.12.

The method is one used in the vicious circle of Stas, and is unfit for anything.

But we have no real direct determinations for potassium, confined to oxygen compounds.

La. LANTHANIUM.

$La_2O_3 : La_2(O_4S)_3 = 326 : 566 = 0.57\ 598$. Chg. 14 high.

These values result from the common La 139. The following is the analytical record:

Hermann, 1861,	3 Det., 690 — 610; 80.	Mean 57 high.
Zschiesche, 1868,	6 Det., 277 — 745; 532.	“ 576 low.
Cleve, 1874,	5 Det., 667 — 590; 77.	“ 22 high.
“ 1883,	12 Det., 500 — 458; 42.	“ 117 low.
Brauner, 1882,	2 Det., 566 — 549; 17.	“ 39 low.
“ 1882,	5 Det., 525 — 451; 74.	“ 117 low.
Bauer, 1884,	4 Det., 569 — 482; 87.	“ 73 low.

These are apparently the best determinations for this element; they show plainly, that the final work has not yet been commenced.

Li = 7. LITHIUM.

$CO_2 : Li_2O_3C = 44 : 74 = 0.59\ 460$. Change 162 low.

Diehl, 1862,	4 Det., 440 — 401; 39.	Mean 43 low.
Troost, 1862,	2 Det., 485 — 427; 58.	“ 4 low.

The value of Troost would be only 0.002 high, or 7.002; but the range is too great.

The other determinations belong mainly to the Stas muddle. We give briefly, first our atomic ratio; also R = range.

0.29 617, $LiCl : AgCl$,	Mallet, 2 Det., R 56.	Mean 27 low.
	Troost, 2 Det., R 45.	“ 24 low.
0.39 352, $LiCl : Ag$,	Stas, 3 Det., R 5.	“ 6 high.
1.62 353, $LiO_3N : LiCl$,	Stas, 3 Det., R 12.	“ 242 high.

Mg = 24. MAGNESIUM. SCHEERER, 1850.

See pp. 108–115 for a full exposition of all that is essential.

Mn = 55. MANGANESE.

$MnO : MnO_4S = 71 : 151 = 0.47\ 021$. Chg. 35 high.
Marignac, 1883, 4 Det., 032 — 987; 45. Mean 14 low.
Weeren, 1890, 6 Det., 005 — 000; 5. " 19 low.
$Ag + MnO : AgO_4Mn = 179 : 227 = 0.78\ 855$. Chg. 9 high.
Dewar and Scott, 1883:
6 Det., 917 — 756; 161. Mean 20 low.

These are the least objectionable methods on chemical reasons. The second shows in execution great range and is atomically dull; the analytical excess represents over 0.2 low.

This leaves the work of Marignac and Weeren, giving a departure of about 0.05 low.

Mo = 96. MOLYBDENUM.

$Mo : MoO_3 = 96 : 144 = 0.66\ 667$. Change 23 high.
Dumas, 1859, Reduct, 6 Det., 741 — 495; 246. Mean 18 low.
Debray, 1868, " 3 Det., 604 — 503; 101. " 111 low.
Rammelsberg, 1877, Reduct, 1 Det., " 41 high.
Seubert and Pollard, 1895:
Reduct, 5 Det., 679 — 661; 18. " 1 high.

$2\ NaCl : Na_2O_4Mo = 117 : 206 = 0.56\ 796$. Chg. 27 low.
E. F. Smith and Maas, 1893:
10 Det., 760 — 733; 27. Mean 51 low

The work of friend Edgar F. Smith, done by Maas, is very good, giving Mo only 0.2 high. But the direct reduction process, in Seubert's laboratory, gives the identical absolute value; only 0.004 high as limit.

N = 14 NITROGEN. LORD RAYLEIGH, 1895.

This most important determination is fully exposed, pp. 159–168.

The old Stasian work on this atomic weight is shown up on pp. 169–198, and the last folly on this topic is presented pp. 169–209.

The determinations of *Julius Thomsen*, of Copenhagen (Oversigt pp. 342–355; 1893), have been used by Clarke (1897, 69), and called "valuable" for nitrogen; hence we must consider them here, though they were made for the ratio O : H.

$HCl : H_3N = 36.5 : 17 = 2.14706$. Chg. 1256 low.

Series	I	11 Det., 160—030;	130.	Mean 613 low.
"	II	5 Det., 130—067;	63.	" 614 low.
"	III	2 Det., 094—081;	13.	" 618 low.

We see here the Stasio-Clarkian demand of concordance to be the sole aim; the range is reduced from 130 to 13. But the constant error remains practically the same, and amounts to about 0.55 on N, making it N = 14.05.

Of course, if the process is correct, it must apply equally well for O or N (see pp. 144–147).

But the process is not good (p. 54), and conflicts with the rule and spirit of Berzelius, who seems to be forgotten in Denmark. Compare p. 84.

Na = 23. SODIUM. Mrs. ASTON, 1893.

The determination of boron by Ramsay and Aston, in 1893, we have utilized for a truly crucial determination of boron, which we have credited to Ramsay, and an excellent determination of Na which is credited to Mrs. Aston.

The precision is certainly within 0.01. See pp. 147–148.

The only other process that needs consideration is the following:

$Na_2O_4S : Na_2O_3C = 142 : 106 = 1.33962$. Chg. 64 low.

Richards, 1891, 8 Det. 005 — 950; 55. Mean 23 high.

The range represents 0.086 on the atomic weight, and the analytical excess 23 high represents 0.036 low on the atomic weight, making it 22.964.

We see that this work of Richards would be strongly confirmatory, if such volumetric work had any great weight at all. Incidentally, this shows how badly all calculations of Richards are muddled and off.

Ni = 58. NICKEL.

The most promising salt to be used appears to be the double salt $Ka_2 O_4 S + Ni O_4 S + 6 H_2 O = 436$. Unfortunately *Sommaruga,* 1866, resorted to the Barium Sulphate process:

Ni Salt: $Ba O_4 S = 436 : 466 = 0.93\ 562$. Chg. 22 high. He made six determinations; 654 — 645: 9. Mean 89 high, corresponding to Ni = 58.4.

Ni : Ni O = 58 : 74 = 0.78 378. Change 29 high.

The analytical excess, from Russell, 1863, to Krüss and Schmidt, 1892, has been brought down from 215 high to 66 high; but this last still represents 0.3 on the atomic weight, high!

O = 16 OXYGEN.

This value is determined by Dumas, 1840; for we take Diamond-Carbon as our *Standard of Matter* (Comptes Rendus, T. 117, pp. 1075-1078; 1893, and True Atomic Weights, 1894, pp. 174-175).

Since for O = 16 we found C = 12 exactly, it follows that adopting C 12 exactly, we obtain O = 16 by the same set of determinations.

Os = 191 OSMIUM.

Seubert, in 1888, made use of the Chloro-Osmiates of Ammonium and of Potassium; the reaction is atomically dull, a change of only about 12 for one-tenth unit on the atomic weight of osmium.

In his second paper, the ammonium salt gives the mean analytical excess 47 high, the potassium salt 74 low.

Hence, the former points to 0.4 high, the latter to 0.6 low.

All we dare say is that 191 will probably prove the true value.

P = 31. PHOSPHORUS. SCHROETTER, 1851.

$P_2 O_5 : P_2 = 142 : 62 = 2.29\ 032$. Chg. 420 low.
Schroetter, 1851, 10 Det., 300—783; 517. Mean 113 low.
Van der Plaats, 1885, 2 Det., 201—072; 129. " 104 high.
Mean of both series, " 5 low.

Schroetter corresponds to 31.027; V. d. P. to 30.975; the mean is 31.001.

0.05 741. $P_2 Ag_{10}$:
V. d. Plaats, 1885, 737 — 726; 13. Mean 9 low.
0.77 327. $Ag_3 : Ag_3 O_4 P$:
V. d. Plaats, 1885, 326 — 300; 26. " 14 low.
0.42 438. $P Cl_3 : 3 Ag$:
Dumas, 1860, 469 — 444; 25. " 17 high.

Pb = 207. LEAD. BERZELIUS, 1810.

This is the first atomic weight established with care and precision, by the father of chemical symbols, chemical language and atomic weights.

See this book, Part II, Chapter II, pp. 74–91.

Pd = 106.5? PALLADIUM.

$Pd : Pd Cl_4 Ka_2 = 106.5 : 326.5 = 0.32619$. Chg. 20 high.
Berzelius, 1828, 2 Det., Mean 70 high.
Joly and Leidie, 1893, 3 Det.:
Rejected by authors, used by Clarke.
" " " 1893, 4 Det., Mean 164 low.
" " " 1893, 2 Det., " 138 low.
$Pd : Pd (N H_3, Cl)_2 = 106.5 : 211.5 = 0.50355$. Chg. 24 high.
Keiser, 1889, Ser. I 11 Det., 383—344; 39. Mean 5 high.
" II 8 Det., 382—343; 39. " 4 high.
Bailey and Lamb, 1892, 10 Det., 218—088; 130. " 184 low.
Keller and Smith, 1892, 9 Det., 519—502; 17. " 153 high.
Ser. I 4 Det., 422—350; 72. " 30 high.
" II 6 Det., 388—360; 28. " 19 high.
" III 4 Det., 430—401; 29. " 59 high.
Keiser and Breed, 1894:
Ser. I 5 Det., 356—339; 17. " 4 low.
" II 4 Det., 360—345; 15. " 16 high.

W. L. Hardin, 1900, uses Diphenyl-Pd-diammon, Cl and Br, also Am Br-Palladate. Claims Pd = 107. Fresen. Ztschr., Bd. 39, p. 541; 1900.

The result points to 106.5, which we might adopt if

Keiser's other determinations had not shown so remarkable fluctuations, as in regard to hydrogen.

But the conflicts are too great.

Pt = 195. PLATINUM. SEUBERT, 1881.

See the excellent analytical determinations of Seubert in Part III, pp. 115–119.

Rb 85.5? RUBIDIUM.

$\text{Rb Cl} : \text{Ag Cl} = 121 : 143.5 = 0.84\ 321.$ Chg. 69 high.

Bunsen, 1861,	4 Det., 388 — 175; 213.	Mean	68 low.
Piccard, 1862,	4 Det., 313 — 245; 68.	"	31 low.
Godeffroy, 1876,	4 Det., 354 — 320; 34.	"	14 high.

The master came within 0.1; hence, no use to credit the value to the closer work of 15 years later.

Other determinations are called for, independent of this silver process.

Rh = 103? RHODIUM.

$\text{Rh} : \text{Rh Cl}_3\,(\text{N H}_3)_5 = 103 : 294.5 = 0.34\ 975.$ Chg. 22 high.
Seubert and Kobbe, 1890:
10 Det., 974 — 929; 45. Mean 21 low.

This is the only series of value, known to me. The work done under Seubert, probably permits the value 103 to be taken.

Ru = 102? RUTHENIUM.

$\text{Ru} : \text{Ru O}_2 = 102 : 134 = 0.76\ 119.$ Change 18 high.
Joly, 1889, 4 Det., 075 — 046; 29. Mean 59 low.

This corresponds to 101.7. All other work is objectionable and very meager. New and serious work is demanded.

S = 32. SULPHUR. MARCHAND, 1844.

See pp. 96–97 for the fundamental determinations, which here are supplemented with some of the more interesting direct determinations.

$\text{Ag}_2\text{ S} : \text{Ag}_2 = 248 : 216 = 1.14\ 815.$ Chg. 14 low.

Dumas, 1860,		5 Det., 838 — 811; 27.	Mean	8 high.
Stas, 1865,		5 Det., 854 — 849; 5.	"	37 high.
Cooke, 1877,	I	5 Det., 892 — 882; 10.	"	73 high.*
	III	2 Det., 823 — 810; 13.	"	15 high.

---

* Rejected for cause.

See True Atomic Weights, 1894, p. 98. Series I *of Cooke*, highly concordant, but greatly in error, because silver lost by volatilization, as Cooke found himself. He instituted the third series under special conditions, avoiding such loss; hence he properly rejected Series I, as we do, and retained III only. The second series was also rejected, for same reasons; in third series reduction took place without visible glow.

We should have stated, that Dumas and Stas operated by actual synthesis, while Cooke reduced pure sulphide in a current of hydrogen.

	$Ag_2 : Ag_2O_4S = 216 : 312 = 0.69\,231.$	Chg.	20 high.
Struve, 1851,	6 Det., 244 — 212; 32.	Mean	1 low.
Stas, 1865,	6 Det., 209 — 197; 12.	"	28 low.

Both series were reductions in a current of hydrogen. Struve used from 5 to 12 grammes of Sulphate, Stas from 56 to 83 grammes. The observations of Cooke explain the difference in the results.

I do not think there will remain a single result of all the much lauded work of Stas after ten years.

Sb = 120. ANTIMONY. SCHNEIDER, 1856.

	$S_3 : Sb_2S_3 = 96 : 336 = 0.28\,571.$	Chg.	16 low.
Schneider, 1856,	8 Det., 559 — 481; 78.	Mean	51 low.
Cooke, 1877,	11 Det.,	"	53 low.
Schneider, 1880,	3 Det., 546 — 534; 12.	"	30 low.

Schneider reduced pure, crystallized Stibnite, from Arnsberg; minute impurities of Ca Carbonate and Fe Sulphide were allowed for. Cooke's work was synthetic, in wet way mainly. Schneider's last value represents 120.2.

The work is very difficult.

While reading the final proof I received from Professor Edgar F. Smith his recent determinations made by heating tartar emetic, dried at 150°, in a current of dry muriatic acid, leaving pure potassium chloride. How the carbon is disposed of, need not here be stated, except that evidently a slight loss of the chloride can hardly be avoided; hence we expect the analytical ratio to come out *low*.

Ka O Sb, $C_4 H_4 O_6 = 323$, Ka Cl = 74.5; hence, Chloride : anhydr. Tartrate = 0.23 049. Change 10 low.

Aided by G. C. Friend, eight determinations were made, ranging from 041 to 049, and from 24 to 16 low; mean analytical excess 17.3 low, corresponding to Sb = 120.17.

Accordingly, this result agrees with the chemical estimate of the process (ratio low, hence atomic weight high).

It is a valuable process, and deserves to be worked with great care.

It is very much to be regretted that only the values reduced to vacuum are given (see pp. 229–230); we cannot rely except on direct weighings.

I am much obliged to Professor Smith for the reprint (from Journal American Chemical Society, XXIII, 502–505; July, 1901), kindly sent me.

Of course, since Prof. Smith uses the false atomic weights of Clarke, he finds Sb = 120.353; since he spares us the probable error, we will overlook the third decimal.

The false Clarke auxiliaries just doubles the error of this work of E. F. Smith.

The other determinations will be given in our abbreviated form; our atomic ratio standing first; R signifies range.

Electrolysis :

1.25 984, $Sb_2 : Cu_3$, Pfeifer, 1881:
3 Det., R 36. Mean 2275 high.

0.37 037, $Sb : Ag_3$, Pfeifer, 1881:
7 Det., R 171. " 448 high.

0.37 037, $Sb : Ag_3$. Popper, ——:
15 Det., R 253. " 397 high.

Silver Processes, gravimetric determinations:

0.63 830, $Sb\,Br_3 : 3\,Ag\,Br$, Cooke, 1877:
15 Det., R 188. " 0

0.71 064, $Sb\,Io_3$, Cooke, 1877, 7 Det., R 209. " 4 low.

Titrations: Dexter, 1857; Dumas, 1859; Kessler, 1861—lead to Sb 122, generally received till 1877. Bongartz, 1883, takes the cake for absurdities; published in Berichte D. Chemische Gesellschaft.

$Se = 79$. SELENIUM. PETTERSSON, 1877.

$Se : SeO_2 = 79 : 111 = 0.71\ 171$. Chg. 26 high.
Sacc, 1847, 3 Det., 161—102; 59. Mean 83 low.
Ekman & Pettersson, 1876, 7 Det., 199—185; 14. " 20 high.
$Ag_2 : Ag_2O_3Se = 216 : 343 = 0.62\ 974$. Chg. 18 low.
Ekman & Pettersson, 1876, 7 Det., 98x—93x; 50. Mean 17 low.
$Hg : HgSe = 200 : 279 = 0.71\ 685$. Chg. 59 low.
Erdmann and Marchand, 1852:
3 Det., 741—726; 15. Mean 48 high.

All other determinations without value. In the above cases, the limit of precision is about 0.1 on the atomic weight.

$Si = 28$. SILICON. THORPE, 1887.

$SiO_2 : SiBr_4 = 60 : 348 = 0.17\ 241$. Chg. 24 high.
Thorpe and Young, 1887:
9 Det., 368 — 324; 44. Mean 106 high.
$SiCl_4 : 4\,Ag = 170 : 432 = 0.39\ 352$. Chg. 23 high.
Pelouze, 1845, 2 Det., 457 — 433; 24. Mean 93 high.
Dumas, 1860, 3 Det., 411 — 340; 71. " 25 high.
$SiCl_4 : 4\,AgCl = 170 : 574 = 0.29\ 617$. Chg. 17 high.
Schiel, 1861, 2 Det., 633 — 592; 41. Mean 4 low.

We credit the determinations to Thorpe, because of the method, giving the oxide which is weighed as such; the substance is unsatisfactory, and the result too wide of mark, 0.4. The problem is very difficult and not settled.

$Sn = 118$. TIN.

$SnO_2 : Sn = 150 : 118 = 1.27\ 119$. Chg. 23 low.
Berzelius, 1826, 1.272.
Vlaanderen, 1858, Reduct, 2 Det. Mean 37 low.
Dumas, 1859, Oxid, 2 Det. " 14 low.
Van der Plaats, 1885, Oxid, 3 Det., 114—091; 23. " 20 low.
Reduct, 4 Det., 117—086; 31. " 17 low.
Bongartz and Classen, 1888:
Electr., 11 Det., Oxidat. " 242 low.

The limit is almost 0.1 for the best determinations.

0.60 185, $SnCl_4 : 4 Ag$, Dumas, 1859, R 17. Mean 22 high.
0.26 941, $Sn : SnBr_4$:
Bongartz and Classen, 1888. " 182 high.
0.28 851, $Sn : Ka_2Cl_6Sn$:
Bongartz and Classen, 1888. " 189 high.
0.32 153, $Sn : Am_2Cl_6Sn$:
Bongartz and Classen, 1888. " 216 high.

The last need no consideration; we have always found Classen's work queer. See Bongartz under Sb.

$Sr = 88$? STRONTIUM.

$6 H_2O : SrCl_2, 6 H_2O = 108 : 267 = 0.40$ 449. Chg. 15 low.
Marignac, 1858, 6 Det. Mean 124 high.
$SrO_4S : SrCl_2 = 184 : 159 = 1.15$ 723. Chg. 10 low.
Marignac, 1858, 3 Det., 949 — 927; 22. Mean 213 high.

These are the only dry way, gravimetric determinations available.

Wet Way, silver process, in short form:

0.73 611 $SrCl_2 : 2 Ag$, Pelouze, 1845:
2 Det., R 15. Mean 133 low.
Change 46 high. Dumas, 1859:
A 4, Det., 34. " 241 low.
B 3, Det., 61. " 201 low.
C 4, Det., 192. " 156 low.
1.14 815, $SrBr_2 : 2 Ag$, Richards, 1894:
I 4, Det., 12. " 132 low.
Change 46 high. II 4, Det., 11. " 125 low.
III 4, Det., 5. " 121 low.
0.65 958, $SrBr_2 : 2 AgBr$, Richards, 1894:
I 3, Det., 3. " 76 low.
Change 27 high. II 4, Det., 6, " 77 low.

This is a most instructive table. We know the chloride process is worse than the bromide process.

Disregarding the very variable results of Dumas on the chloride, we may say that $Sr = 87.73$ according to Richards.

For Sr as mean between Ca and Ba, we should have 88.5.

The only positive result is that the atomic weight of Strontium remains unknown.

Te. TELLURIUM.

We will make use of this case to show how closely this work comes under the determination of the most probable formula in ordinary chemical investigation.

The atomic weight of tellurium having been much in doubt, presents a favorable case for this parallel.

We shall limit ourselves, as ordinarily, to dry way processes with oxygen compounds.

Telluric Oxide, $Te O_2$, and crystallized Telluric Acid, $H_6 O_6 Te$ are the only compounds to be considered. We give the atomic ratios for Te from 124 to 128.

Te.	Te : Oxide.	Oxide : Acid.	Te : Acid.
124	0.79 487		
125	618	0.69 163	0.55 066
126	747	298	262
127	874	432	458
128	0.80 000	566	652
Chg.	13	13	20

The change for 0.1 is given to nearest unit only.

Te : $Te O_2$, Analytical Determinations:

Berzelius, 1833,	3 Det., 057—034; 23.	Mean	0.80 042
Wills, 1879, I Series,*	5 Det., 207—828; 379.	"	015
II Series,	4 Det., 040—012; 28,	"	028
Brauner, 1889,	5 Det., 798—625; 173.	"	0.79 711
	1 Det., by reduction.		932
Staudenmaier, 1895,	4 Det., 966—935; 31. by reduction.		950
Same, Oxide : Acid,	7 Det., 553—429; 124.		0.69 440
Same, Te : Acid,	3 Det., 518—488; 30.		0.55 508

These determinations, reduced by the usual method of

---

* Oxidized by Nitric Acid, which is known to be an imperfect agent or tellurium; hence great range.

mental arithmetic to atomic weights, using the change per 0.1 stated, give the following very varied values:

Chemist.	Te : Oxide.	Oxide : Acid.	Te : Acid.
Berzelius,	128.3		
Wills, I	128.1		
II	128.2		
Brauner, 5	125.7		
1	127.4		
Staudenmaier,	127.4	127.05	127.25

What is needed, is much more careful work. Brauner intended to find low, for "periodic law."

The "face of the returns" shows that tellurium has a higher atomic weight than iodine; possibly 128 is the value, and old Berzelius was right again.

Ti = 48. TITANIUM. H. ROSE, 1823.

$TiO_2 : TiCl_4 = 80 : 190 = 0.42\ 105$. Chg. 31 high.
H. Rose, 1823, 5 Det., 825 — 273; 552. Mean 828 high.
Demoly, 1849, 3 Det., " 3811 low.
Thorpe, 1883, 6 Det., 182 — 160; 22. " 66 high.
Process by water alone; oxide weighed.
$TiO_2 : TiBr_4 = 80 : 368 = 0.21\ 739$. Change 21 high.
Thorpe, 1885, 3 Det., 790 — 762; 28. Mean 36 high.

To Silver:

$TiCl_4 : 4\,Ag = 190 : 432 = 0.43\ 982$. Change 23 high.
Isidore Pierre, 1847, 9 Det., 520 — 322; 198. Mean 450 high.
Thorpe, 1883, 8 Det., 015 — 978; 37. " 17 high.
$TiBr_4 : 4\,Ag = 368 : 432 = 0.85\ 235$. Change 23 high.
Thorpe, 1885, 5 Det., 241 — 230; 11. Mean 50 high.

To Silver Haloid:

$TiCl_4 : 4\,AgCl = 190 : 574 = 0.33\ 101$. Chg. 18 high.
H. Rose, 1823. 5 Det., 258 — 100; 158. Mean 55 high.
Demoly, 1849, 3 Det., " 1480 high.
Thorpe, 1883, 5 Det., 125 — 111; 14. " 17 high.
$TiBr_4 : 4\,AgBr = 368 : 752 = 0.48\ 936$. Chg. 13 high.
Thorpe, 1885, 4 Det., 982 — 951; 31. Mean 26 high.

The so-called determinations of Demoly are a curiosity;

of course, Clarke puts them into his Olla, and intimates that it had no appreciable influence on the odor (p. 193). Very natural.

By silver chloride, Rose's 55 were brought down by Thorpe, 60 years later, to 17; in atomic weights, 48.3 to 48.1. Hence, it seemed proper to leave the old hero's name, who devised both processes, and did so much other work in the spirit of his teacher, Berzelius.

The work of Thorpe is very excellent, as it is for Si. By the change the departure is easily estimated; generally above 0.1.

Tl = 204. THALLIUM. CROOKES, 1873.

See pp. 120–137 for details on this most sensational case of atomic weight determinations.

Ur = 240. URANIUM. EBELMEN, 1842.

$O_2 : 3\, UrO_2 = 32 : 816 = 0.03\,916$. Change 15 low.
Ebelmen, 1842, 5 Det., 949 — 867; 82. Mean 3 low.
Zimmermann, 1886, 10 Det., 929 — 925; 4. " 12 high.

Here we have the contrast between the "old" and the "new." Great fear of lack of concordance, due to whip of "probable error," which in turn is to give "high weight" to the work of the chemist.

Then the oracle (Clarke p. 266) will say: "In short, "Ebelmen's mean vanishes when combined with Zimmer-"mann's;" for we find the probable error of the first 0.0090, of the latter 0.0003, or *one-thirtieth* only.

Hence, Zimmermann is 30 × 30 = 900 times more weighty as a chemist, and one of his data counts as much as 900 of Ebelmen! How simple and how scientific the work of this oracle. In these proportions everything goes into his olla podrida.

Has any modern chemist ever protested against this horrible treatment meted out to our predecessors by that scientific pasha at Washington? Has Crookes protested, that Ebelmen is dead?

And now, when we test these determinations by our standard—which has been firmly established—we find the work of Zimmermann four times as far off as that of Ebelmen, determined almost half a century earlier.

Out upon these "Official—Scientists" that use their position in Washington, as do the worst politicians, to build up a following in the American Chemical Society and to subdue the American Association Adv. Sc.

The work of Wertheim, 1843, consisted in igniting the double acetate of Ur and Na, giving Na Uranate. The starting salt is too complex; atomic weight 472, entering twice, or as 944. Resulting $Na_2 O_7 Ur_2 = 638$. Atomic ratio 0.67 585, change 7 high. Results:

Wertheim, 1843,	3 Det.,	546 — 509; 37.	Mean 62 low.
Zimmermann, 1886,	4 Det.,	557 — 540; 17.	" 33 low.

What is particularly striking is the very low value 7 of the change per 0.1; i. e. in our language, the process is atomically very dull.

The oxalate method of Wertheim is still inferior. No reliable results can be obtained by chosing too complex a compound as starting substance, and working it towards a very blunt ratio.

The recently proposed method of Armand Gautier, worked by J. Aloy, has already been mentioned because the author has withheld the most essential data of observation, and only stated the results found by adopting the false value $N = 14.04$. See pp. 35–38.

But we are given the weight of a cubic centimeter of nitrogen, according to Lord Rayleigh—to the millionth of a gramme and less, by simply pointing off this great experimentors result per Litre. This looks very fine, indeed—"exact science" truly.

The method itself is very inferior, since the weight of nitrogen is multiplied by 17 and a fraction; this magnifies the errors of nitrogen determination by seventeen!

Indeed, I doubt if Armand Gautier would have presented such a method if he still had the privilege of talking over atomic weight determinations with Schützenberger and Friedel, as he was want to do when considering my work.

To adopt the weight of nitrogen per unit of measure from Lord Rayleigh, and then use the false Stas value for the atomic weight of nitrogen, disproved by the same Lord Rayleigh, is almost too grotesque even for his present associate Moissan.

Making a blunder of 0.7 at the start does not augur well for the outcome. Since all essential data of experiment are withheld, we cannot tell how many other faux pas are thus fortunately hidden from our view.

When Moissan slavishly adopts the false atomic weights of the German Chemical Society we need expect no results worth consideration from French Chemists. See p. 34.

Va = 51. VANADIUM. ROSCOE, 1868.

$O_2 : Va_2 O_5 = 32 : 182 = 0.17\ 582.$ Chg. 19 low.
Roscoe, 1868, 5 Det., 533 — 589; 44. Mean 73 low.

This corresponds to 0.38 high or 51.38. The experimental results are evidently low, for some reason.

The volumetric work given below, need not be considered. What is wanted, is a thorough revision of the dry way work.

$Va\ O\ Cl_3 : 3\ Ag = 173.5 : 324 = 0.53\ 545.$
Roscoe, 1868, A : 6 Det., 533 — 425; 108. Mean 35 low.
B : 3 Det., 980 — 479; 501. " 41 high.

$Va\ O\ Cl_3 : 3\ Ag\ Cl = 173.5 . 430.5 = 0.40\ 302.$
Roscoe, 1868, A : 6 Det., 537 — 174; 362. Mean 158 high.
B : 2 Det., 391 — 333; 58. " 60 high.

Wo = Tu = 184. WOLFRAM. SCHNEIDER, 1850.

$Wo : Wo\ O_3 = 184 : 232 = 0.79\ 310.$ Chg. 9 high.
Schneider, 1850, Reduct, 5 Det., 350—254; 96. Mean 6 high.
Oxidat, 3 Det., 329—324; 5. " 17 high.
Marchand, 1851, Reduct, 2 Det., 307—302; 5. " 5 low.
Oxidat, 2 Det., 352—321; 31. " 26 high.

v. Borch, 1851,	Reduct, 7 Det., 313—212;	101.	Mean	33 low.
	Oxidat, 2 Det., 359—339;	20.	"	39 high.
Dumas, 1860,	Reduct, 8 Det., 389—259;	130.	"	2 high.
Bernoulli, 1860,	Impure materials.			
Persoz, 1864,	Reduct, 2 Det., 324—304;	20.	"	4 high.
Roscoe, 1872,	Reduct, 3 Det., 308—196;	112.	"	47 low.
	Oxidat, 2 Det., 299—230;	69.	"	45 low.
Waddell, 1886,	Reduct, 5 Det., 362—311;	51.	"	29 high.
Pennington and Smith, 1894:				
	Oxidat, 9 Det., 394—390;	4.	"	82 high.
Shinn, 1896,	Oxidat, 4 Det., 417—377;	40.	"	81 high.
Schneider, 1896,	Reduct, 3 Det., 323—307;	16.	"	3 high.
	Oxidat, 3 Det., 314—304;	10.	"	0 high.
Smith and Desi, 1894:				
	Reduct, 6 Det., by weighing H_2O formed.			

The last method, introducing the questionable value of hydrogen, must be excluded. Besides, it varies enormously in different experiments for about the same amount of material used. This series must be definitely thrown aside.

There next remains the other two series made by and for Professor Smith, giving 81 and 82 high, or 0.9 in atomic weight, high. It would, standing by itself, give 185. It is claimed that the material was specially free from Molybdenum.

Another fact is the extreme accuracy of weighing—to the thousandth of the milligramme! Where such a feature is prominent, I have usually found serious errors in important matters. Such spurious accuracy throws doubt on essential points, at least in my mind.

I have had no means of seeing the unreduced weighings. The reduction to vacuum is claimed to have been made; *how*, that is the great question. Compare what happened in Smith's laboratory with the reductions of arsenic (see p. 230). I fear that systematic errors lurk right here.

The other determinations all agree reasonably well.

Oxidation gives uniformly higher results than reduction, except in the case of Roscoe, where reduction is abnormally low.

It may be best to summarize all results as follows, in three plainly marked groups:

	Reduct.	Oxidat.
Schneider, 1850,	6 high.	17 high.
" 1896.	3 high.	0 high.
Marchand, 1851,	5 low.	26 high.
Dumas, 1860,	2 high.	
Persoz, 1864,	4 high.	
Mean,	2 high.	14 high.
v. Borch, 1851,	33 low.	39 high.
Roscoe, 1872,	47 low.	45 low.
Waddell, 1886,	29 high.	
Mean,	16 low.	3 high.
Pennington and Smith, 1894,		82 high.
Shinn, 1896,		81 high.
Mean,		81 high.

In the first group, we have Ernst Richard Schneider as a masterly analyst, who has proved his Berzelian school in his work on Bi and Sb. Marchand stands equally high. See his work on Hg, S, Ca; he started Scheerer on Mg. Dumas, in the dry way, is a master; see his diamond and calcite work. We conclude that this group is right. For reduction, this mean gives 184.02; for oxidation, 184.15, which is throughout the less reliable.

Concerning the second group, we have no evidence that Roscoe did the work; probably two chemical students, called "analyst A" and "B" did the work. The other two analysts are not known by any other work of this kind, good or bad. Hence, this group possesses no weight.

There remains the third group, comprising oxidation work done by or under Prof. Smith.

We have already stated why we cannot accept his record in this case. The process by weighing the water was decidedly imperfect in execution—the curve of errors running straight up. We have on record bad breaks in reduction to vacuum (see pp. 229–231) and from the formula used (see p. 175) need not wonder thereat. The excessive accuracy in weighing (*fancied*, and drilled into that institution through Professor Barker) must detract from essentials.

When Professor E. F. Smith, in 1899, (Journal American Chemical Society, 1899, p. 1022), concludes, "it is evident "that no reliable atomic *mass* determinations can be made "by the *oxidation* of metallic tungsten" we may agree, if he means the atomic *weight;* we have really used reduction results, only.

But his *reason* stated looks very insufficient; (l. c., p. 1022). We need only put it into plain form, in milligrammes. We find a loss of 1.0 per process, a round—and it is no doubt due to the oxidation part of the round.

For these—and other reasons—we must decline to consider the work of Smith and his students of any value in comparison to the work summarized in our first group, done by known and proved masters.

There is one other reason, which I have not stated, and which others cannot as fully appreciate as I have learned to do. It is the high praise from Clarke, see page 259 (extremely low probable error); p. 260. (P. and S. vastly outweighs everything else); p. 263 (work in Smith's laboratory dominates all the rest).

Knowing, by long experience, that Clarke's opinion, being based on a false formalism, is not only worthless, but invariably on the side of error, I see this, my opinion, most strikingly verified in this instance.

If there is a case in which this author of weighing and judging chemical work is right (Clarke), I should be delighted to have it pointed out to me.

Zn = 65. ZINC. AXEL ERDMANN, 1844.

$\mathrm{Zn} : \mathrm{Zn\,O} = 65 : 81 = 0.80\,247$. Change 24 high.

Jacquelin, 1842:

Wet way, 4 Det., 570 — 524; 46. Mean 296 high.

Axel Erdmann, 1844;

Dry way, 4 Det., 274 — 247; 27. " 13 high.

Morse and Burton, 1888:

15 Det., 320 — 305; 15. " 65 high.

Recent work on absorption of oxygen by the oxide, has not been sufficiently studied, not having had access to the details.

The work of Axel Erdmann was undertaken at the request of Berzelius, and reported by him to the Swedish Academy, and in his Annual Report, March 31, 1844, (French Series, transl. Plantamour, No. 5, p. 71; Paris, 1845). He found it necessary to use porcelain crucibles.

The entire range is only equivalent to 0.1 on the atomic weight, while the analytical excess of the mean represents only 0.05 high. Crucible work in those days was quite different from what it is now.

From all I can tell, by looking carefully over the entire mass of work done, I must conclude this to be the true value.

The determinations made since 1844 have been very numerous, especially in more recent years.

We have even a goodly number of determinations by students, called "practice work of students;" but such work should remain sacred to the laboratory, and not be published as atomic weight determinations (under Morse and Keiser, in Johns Hopkins University, Baltimore, containing 51 determinations; see Clarke, p. 150, who puts the mean in his Olla Podrida, of course).

All determinations by means of hydrogen must, at present, be ruled out, because of the uncertainty affecting the latter. This excludes Van der Plaats, 1887, Mallet, 1890; and the student's work.

Of the wet way processes, we must note the most pretentious by Richards and Rogers, of Harvard University, which point to 65.5 as the atomic weight of Zinc. We calculate for this value 65.5:

$Zn\,Br_2 : 2\,Ag = 225.5 : 216 = 1.04\,398$. Chg. 46 high.

Series A	No Ag determination.	
" B	4 Det., 411 — 376; 35.	Mean 6 low.
" C	3 Det., 380 — 377; 3.	" 19 low.

$Zn\,Br_2 : 2\,Ag\,Br = 225.5 : 376 = 0.59\,973$. Chg. 26 high.

Series A	5 Det., 984 — 961; 23.	Mean 2 high.
" B	4 Det., 977 — 959; 18.	" 6 low.
" D	3 Det., 962 — 961; 1.	" 12 low.

Richards sacrifices everything to concordance, as well known, and specially striking in the present work. In the silver process, he reduces the range to one-tenth, but increases the analytical excess threefold. In the silver bromide process, he reduces the range from 23 to 1, while the analytical excess is lowered from 2 high to 12 low!

I must confess that I have, for some time, thought this work reasonably reliable, on account of its being done with bromine, and not with chlorine.

But upon carefully looking over the entire field, and remembering the ordinary deviation of this wet way work (which for the most concordant brings it 0.05 below) I am compelled to drop this as fictitious, and to adopt the dry way work of Axel Erdmann done under the old master's eye, before modern fancy methods got a start.

Indeed, the record of the determination of the atomic weight of zinc, since 1850, is a disgraceful one; even Marignac, in 1883, went astray.

It is to be hoped, that strictly rational, common sense work of revision will be done for this metal at an early day.

Zr = 90. ZIRCONIUM.

$ZrO_2 : Zr(O_4S)_2 = 122 : 282 = 0.43\ 262.$ Chg. 21 high.
Berzelius, 1825, 6 Det., Mean 128 low.
Weibull, 1881, 7 Det., 321 — 081; 241. " 112 low.
Bailey, 1889, 8 Det., 402 — 337; 65. " 110 low.
$ZrO_2 : Zr(O_4Se)_2 = 122 : 376 = 0.32\ 447.$ Chg. 18 high.
Bailey, 1889, 5 Det., 640 — 470; 170. Mean 111 high.

It is apparent that the results from the selenate would raise the atomic weight as much as those from the sulphate would lower it, namely, 0.5. We, therefore, leave it at 90.

ADDENDA TO PART SECOND.

I. REDUCTION TO AIR.

The Reduction to Vacuum was condemned by Berzelius. He considered it a mere "gnat" in comparison to the unavoidable errors of all other operations involved.

We have found this opinion of Berzelius to be exactly true.

Besides, we have shown that this pretended "correction" of the experimental determinations de facto introduces gross errors in practice.

We will here only refer to the errors of 12 and 66 milligrammes "inadvertantly" committed by Stas in Nos. 8 and 6 of his famous syntheses of silver nitrate. See pp. 175–176.

We may also recall the remarkable "rise" in Nos. 5 and 8 of the determinations of arsenic, though the reduction of necessity involves a lowering of the ratio. See p. 230.

By a most astonishing play of fate, in all these cases of "inadvertency" the result was a "smoothening of the curve," a most notable diminution of the deviations from a constant mean, a truly wonderful diminution of residual "errors."

How strange that such inadvertent errors should reduce the actual errors so as to reduce the final probable error of the mean!

We have strongly advised to leave the weighings without reducing them to vacuum.

We must insist on giving the *actual weighings in air* in all cases, even if the weighings reduced to vacuum are given also.

The former are the true results of observation; the latter we have seen to be often affected with very large errors, due to errors of calculation—or other causes.

I shall here show how superfluous this entire reduction to vacuum is, and how the exact comparison can be made by the following most simple process of calculation.

This process consists in calculating the *change of our absolute atomic ratio due to the buoyancy of the air.*

In other words, we prefer to reduce our absolute weights to air, the very opposite of the common process.

By the well known method, reduction to vacuum there is a certain small factor k milligramme per gramme to be added to the apparent weight or the weight in air.

Consequently, the apparent weight will be obtained from the absolute weight, by *subtracting* this small amount.

These factors have been tabulated by Kohlrausch (Leitfaden, IV Aufl., 1880, p. 286; also Ostwald, Hand und Hilfsbuch, 1893; p. 47; also my General Chemistry, 1897, pp. 220 and 231).

For a quotient, the minute correction is obtained by subtracting the correction of the divisor from that of the dividend, as is well known.

Let us apply this process to the case of arsenic.

The dividend is 234, the absolute weight of the salt, formed from 354 of pyroarsenate, the divisor.

The specific gravity of salt is 2.16, which gives its factor 0.41 milligrammes per gramme, allowing for brass weights.

The specific gravity of the pyroarsenate was (by Hibbs) taken at 2.295 for which we take 2.30; this gives the factor 0.38 milligrammes per gramme.

Reducing *to air*, makes the sign opposite; hence resulting factor for the fraction is

$$-0.41 - (-0.38) \text{ which is } -0.03,$$

that is a *subtraction* of 0.03 milligrammes per gramme, or 3 units in the fifth place.

But the ratio is 0.66 102 for which we take here $0.6\bar{6}$, that is ⅔.

Now ⅔ times 3 is 2; the sign being negative, we see that in this case

> *the reduction of our absolute atomic ratio to air is obtained by adding two units in the fifth decimal.*

If now, we wish to reduce the actual weighings given p. 207 to vacuum, we simply need to *subtract* two units in the fifth place.

The table, p. 230, will now show how remarkable are the errors of reduction actually committed.

Of course, we must retain our absolute atomic ratios pure and simple, and merely calculate this reduction to air in the manner shown to be able to allow for actual differences observed.

As the slight variations produced by changing temperature and pressure fix only the correction of a correction, that is, a minute quantity of second order, they are usually entirely insignificant. Sufficient data for the allowance are given in my General Chemistry, 231, note.

To complete this subject, we state how the minute factor referred to above is actually obtained.

The weight of one cubic centimeter of air, at common temperatures and pressures, is 1.2 milligrammes.

If S is the specific gravity (i. e., grammes per cc), then the specific volume is 1 divided by S (cc per gramme).

Hence the weight of one gramme of this substance is buoyed up by 1.2/S milligrammes.

The *brass weights* (spec. grav. 8.5) are, therefore, buoyed up by 0.14 milligrammes per gramme. As they are on the opposite pan, it must be subtracted from the buoyancy of the substance weighed.

For Salt, S = 2.16 gives 1.2/S = 0.55; corrected for brass weights 0.55 — 0.14 = 0.41.

For Pyroarsenate S = 2.30 gives 1.2/S = 0.52; corrected for brass weights, 0.38.

These are the minute factors above used.

In this way, a most simple calculation will quickly show, how many units in the fifth place the absolute atomic ratio will be lowered or raised by the buoyancy of the air.

No calculations are to be made for each individual determination; only *one* single and most simple calculation for the entire process. See pp. 64–65.

If this direction is followed to the letter, there will not be any chance for such deplorable inadvertent mistakes in the reduction to vacuum as have disfigured the work of Stas and his disciples—until we lifted the veil of mystery and fraudulent exactitude that has hid them for so many years.

II. HOW CROOKES MANUFACTURED DECIMALS.

In the absence of the Philosophical Transactions (see p. 122), I supposed that the last three decimals had been "determined" by Crookes by the oscillation method (p. 129).

Looking at a book in the hands of my son, taken by him from the Mercantile Library, I find it to be Crookes' Select Methods in Chemical Analysis, II edition, London, 1886.

Opening this volume, I see near its close pages with many decimals (pp. 687–691) which naturally attract my attention.

I discover with joy a part of Crookes' Memoir on the atomic weight of thallium, reprinted from the Philosophical Transactions of 1873.

In a few minutes I learn how Crookes actually obtained his wonderful weighings to the millionth of the grain, by reading *how he obtained the more wonderful weighings to the thousand-millionth of a grain.*

Das geht denn doch über das Bohnenlied!

Crookes did not tamper with the individual weighings; he falsified his weights.

The Modus Operandi.

Mr. William Crookes, in the summer of 1864, adjusted and tested an evidently fine set of platinum grain weights (l. c., p. 686).

The testing was done according to the ordinary method, taking the largest weight as standard of comparison. This is a thousand grain weight (about 65 grammes).

Since 1864 he has repeatedly tested these weights; "they have shown up to the present time, absolutely no alteration" (p. 691).

"The present time," is 1873, when that paper was printed in the Philosophical Transactions.

Since it is reprinted by Mr. William Crookes, in 1886, without dissent or annotation, we are entitled to conclude that his set of platinum weights had shown "absolutely no alteration" even to 1886, that is in 22 years.

It is surely a most excellent set of weights, and Mr. Crookes has handled it most carefully.

On page 688 we find an important item about the balance, apparently the very one "specially constructed for that research." (see p. 122, supra). I must quote this footnote, for I want to be fair, always; the italics are ours.

"Although these decimals are carried to the sixth place, "*the balance would not indicate beyond the fourth place.* By "taking the mean of ten interchanged weighings, I could "obtain a fifth place. The calculated values of the weights "were carried to the sixth decimal, in order to avoid inaccuracy in the fourth and fifth places when several values "were summed."

If Mr. William Crookes had strictly carried out this intention, and *in the published results* dropped the last two places, he would have only subjected himself to needless drudgery.

But on page 691 he gives a table of the individual weights of his fine set to the sixth place, that is to *the millionth of a grain;* and there can remain no doubt but by this table he summed up his actual weighings, and obtained the millionth of the grain in his published results, requiring "expurgation" at our hands, see p. 128, supra.

The *fact* that "Crookes continued calculating till he got tired, and did not get tired till he had passed the limit of precision by three places beyond the ken of his balance," as we surmised (pp. 129-130, supra) remains.

According to his own footnote he passed it by *two* places.

Instead of using the oscillation method, he simply used his actual weights to the *hundredth* of a grain (⅔ mgr.) and a rider of this weight.

All the decimals beyond this—and there are *four* of them—are the result of his weighing of his weights, the comparison of his actual set of platinum weights, by means of his balance "that would not indicate beyond the fourth place."

He has also adjusted a rider of 0.01 grains, which would give him the fourth decimal, which his balance will "indicate," but not determine.

How very readily Mr. William Crookes takes decimals is shown in a striking manner in the extract from his paper in question, reprinted in his "Select Methods" of 1886, before me.

The 1000 grain weight he balances by the 600 + 300 + 100 and 0.01 grain; that is solid work, to the hundredth of a grain (= ⅔ mgr.).

His 600 grain weight is determined to *five places,* as are all other weights; that is, the balance responds only to the fourth, but 10 trials give him a *mean* to the fifth.

According to Crookes, there are no constant errors; the *mean* is right. See p. 46, supra.

In his calculations he does not tire (see p. 130, supra). Up to Equation E he gives 5 decimals; but from F he already gets 6, from K he gets 7, from P he gets 8, from U he gets 9 and keeps getting 9 decimals till the last Equation, Z.

Thus, the "exact weight" of his rider is given (l. c., p. 690.) 0.009 996 997 grain.

To use *this* figure instead of 0.010 is the manifestation of the most colossal stupidity; for it asserts the actual tangible determination of 0.000 003 003 grains by means of a balance giving only 0.001 with certainty.

To give the weight of a platinum wire to the thousand-millionth by weighing it to the thousandth on a balance barely sensitive to the ten thousandth, is a feat to which nothing in modern science is comparable.

We have to go back to the days of Isaac Abensid (Hassan), under Alphons, King of Castile, some seven centuries ago to match it, when the daily motion of the moon was given to 8 sexagesimal places. Mädler, Geschichte, I, p. 101; 1873.

In this manner, by numerical elimination, grinding out with pencil on paper from 5 to 9 decimals of the *grain*, Crookes weighed to the ninth decimal of the grain on his balance!

We must be profoundly thankful, that Mr. William Crookes did not work in all of his 9 decimals, but in his table chopped off the last three.

In this way he "weighed his weights" to the millionth of the grain.

He "summed" this value of his weights on the pan—and, of course, obtained the weight of his thallium and its nitrate to the millionth of the grain!

And I had supposed that Mr. William Crookes "determined each weighing independently, by the oscillation method!

But the fraud and the imposition remain the same—only a little more so, because of the greater clumsiness of the process.

CONCLUDING REMARKS.

I. THE DEGREE OF CERTAINTY ATTAINED.

Having completed the exposition of the actual analytical determinations made during the century just closed, and having also given a final reductio ad absurdum of the system of Stas, pp. 169–209, we must close the entire work with a numerical summary of the evidence of the exact commensurability of the true atomic weights of the chemical elements and of the unity of matter.

The manner of procedure in this demonstration has been fully explained on pp. 212–214. We may repeat the method here in more general terms.

If the measure of precision of the true atomic weight be p which is the n^{th} part of a unit, then the certainty is as n to 1 for one, n^2 to 1 for 2, n^m to 1 for m elements all terminating their true atomic weight with the full natural number.

Is $p = 0.001$ then $n = 1000$; for 5 such elements, n^5 is one followed by $5 \times 3 = 15$ ciphers.

We shall now enumerate the elements in groups according to this degree of precision.

I. For the following eleven elements the precision of determination of this absolute atomic weight reaches 0.001; hence, one thousand divisions of the unit giving for all the termination .000, represents one chance in 1000 raised to the eleventh power, which is expressed by the number one followed by 33 ciphers.

The eleven elements here referred to are: Ag, As, Bo, Br, C, Cd, Cu, N, O, P, Tl.

We could also say, that this number

1000 000000 000000 000000 000000 000000

compared to one represents the *certainty* that these eleven elements are composed of but one primitive material, the atomic weight of which is one twenty-fourth of that of carbon-diamond. We have called this primitive material PANTOGEN.

II. For the three elements, Fl, H, Li, the precision does not exceed 0.002, of which 500 make one unit. Hence, the corresponding factor of certainty is 500 in the third power, which is 125 000000.

III. For the two elements, Io and Mo, the precision is only 0.005, of which 200 make one unit. Hence, the corresponding factor of certainty is 200 in the second power, or 40 000.

IV. For the following nine elements Ca, Cl, Fe, Hg, Mg, Na, Pb, Pt, S, the precision attained is 0.01 of which 100 go to a unit. The corresponding factor of certainty is, therefore, 100 raised to the ninth power, that is

1 000000 000000 000000,

or one followed by 18 ciphers.

The certainty of the entire result is the product of these factors, tabulated below. The single factors are resolved in the numerical part and the number of ciphers following, so that the final product can be most readily determined.

We also once more enumerate the actual individual elements of each group.

Group.	Precision.	No.	Elements.	Factor.	Ciphers.
I	0.001	11	Ag, As, Bo, Br, C, Cd, Cu, N, O, P, Tl,	1	33
II	0.002	3	Fl, H, Li,	125	6
III	0.005	2	Io, Mo,	4	4
IV	0.01	9	Ca, Cl, Fe, Hg, Mg, Na, Pb, Pt, S,	1	18
	Total:	25	Elements,	500	61

or 5000 followed by 60 ciphers,
or 5000 followed by 10 times the million group of 6 ciphers.

This number is exactly five thousand times the greatest number ever formulated by antiquity, namely, by Archimedes of Syracuse, in his Arenary or Sand-Calculus. He obtained (in our notation) the number 1 followed by sixty ciphers, by estimating the number of grains of sand that could be contained in the entire universe as then known. See the admirable French translation of his works, by F. Peyraud, Tome II, p. 231-264; Paris, 1808.

We may consider this number practically infinite, under all ordinary human conditions.

Hence, we can say, that the twenty-five chemical elements enumerated, for which the true atomic weight is known with a degree of precision of at least 0.01 of a unit, are hereby proved to be compounds of one single material, the atomic weight of which is *one-half a unit*, or $\frac{1}{24}$ of our standard of matter, the diamond-carbon.

The chance that this conclusion is an error is as one is to the practically infinite number just given, namely, 5000 followed by ten times the million group of six ciphers.

We can also state the same conclusion by saying its *certainty* is expressed by this same number, the chance of an error being unity.

Since now these twenty-five chemical elements were not chosen or selected by us for any special reason other than the accuracy or the degree of precision wherewith their atomic weight has been determined, this conclusion applies to any and every group of 25 chemical elements, that is, to *all chemical elements*, even those not yet known to us.

These 25 elements comprise fully one-third of all elements known to-day. They include nearly all the best known and most important of these chemical elements.

As soon as the atomic weight determinations shall be carried on in a rational manner, in accordance with the principles laid down in this work and in our True Atomic Weights of 1894, the required precision will be obtained and the elements will fall into line.

It is easily determined that the certainty for all 38 elements now determined with a precision of 0.1 or more is

800 followed by thirteen times the million-group of 6 ciphers,

which is 800 000000 000000 000000

times the greatest number of antiquity of Archimedes.

The thirteen *additional elements* here referred to are:

V Al, Bi, Ur, precision 0.02;
VI Au, Be, In, Mn, Se, Wo, Zn, precision 0.05;
VII Sb, Sn, Ti, precision 0.1.

II. THAT STELLAR HAYSTACK.

Since we have shown (p. 213) that for the 12 elements the certainty was 1 followed by four times the million group of six cyphers, it follows that

> *the certainty of 38 elements* is 800 followed by nine times the million group of six ciphers

times as great

> as for the 12 elements referred to.

Since the solitary needle in the haystack covering a globe a thousand times the radius of our earth, was found to represent the one chance that we are wrong in our conclusion, based upon the determinations of twelve elements, we must try to find the base of the haystack corresponding to the chance of error for the 38 elements here considered.

Since the square root of 800 is a little over 28, it follows that the square root of the last number given is

> 28 000 followed by *four* times the million group of six ciphers.

Accordingly, the radius of the globe having the necessary surface to hold the haystack for the 38 elements is this number of times the thousand radii of the earth which formed the base for the haystack for the dozen elements; p. 217.

In other words, the radius of the globe having the necessary surface to hold our 38 element haystack is

> 28 followed by *five* times the million group of six ciphers times the radius of our earth.

The sun's distance from the earth is 23 150 times the radius of the earth.

Dividing this into the above, we get (enlarging the divisor to 28 000 for convenience of division, and to strengthen our result):

> The globe having a surface equal to the haystack for 38 elements must have a radius over 1000 followed by *four* times the million group of six ciphers times the earth's distance from the sun.

Since the distance from the sun to Neptune, the most distant planet known, is only 30 times as great as the distance from the sun to the earth, the radius required passes far beyond our planetary system into stellar space.

The researches on the parallax of the stars have shown, that stars like 61 Cygni and a Centauri are distant less than half a million times the distance from the earth to the sun.

At a million times the distance—earth-sun—we, therefore, are beyond the distance for which it has been possible to estimate stellar distances by the most refined astronomical researches.

But the radius of the globe to hold our 38 element haystack must be

1 000 000000 000000 000000

times as large as this distance of the ordinary stars!

From such a distance no light has ever reached human eye, even by means of the great speculum, six feet in diameter, of the Earl of Rosse at Parsonstown, Ireland.

We give it up. We cannot convey any tangible conception of the number presented. It is infinite for the mind of finite man.

But if the haystack has a base inconceivably larger than the stellar world visible to us, and if the chance of our conclusion being in error is no greater than that of finding a single needle in this infinite haystack, may we not say that our conclusion is proved true with greater certainty than any other scientific conclusion ever drawn about nature!

We, therefore, are entitled to state this conclusion once more in words (see pp. 217 and 218):

> The atomic weight of all chemical elements are exactly commensurable;
>
> the greatest common divisor of all is half a unit, the atomic weight of carbon-diamond being taken at 12 exactly;
>
> therefore, the atoms of the chemical elements are composed of but one kind of primitive atoms, of pantogen, the atomic weight of which is exactly half our unit; and
>
> the great majority of element-atoms consist of an even number of such pan-atoms.

III. TABLE OF ATOMIC NUMBERS.

Hence also the *number of atoms of pantogen* contained in *one atom* of any given element, is exactly *two times* the true atomic weight of that element, the atomic weight of carbon-diamond being taken at 12 exactly.

This number we call the *atomic number* (Atomzahl in German); we have also called it atogramme in our Programme of 1867. Compare pp. 217–218, supra.

While this is not the place for the study of this most important subject, which we shall take up in another work, we deem it sufficiently interesting to give a *table of atomic numbers* for the most important chemical elements.

The arrangement and order of the elements, we take from Part III, pp. 205–256, of our True Atomic Weights, 1894, and from pp. 200–201 of our Introduction to *General Chemistry*, 1897.

Table of Atomic Numbers.

I. THE CARBON SYSTEM; from Positive to Negative.

Valence.	Name.	Symbol.	First.	Second.	Third.	Fourth.	Fifth.
1	Kaloids,	*Kα*	Li 14	Na 46			
2	Cadmoids,	*Kδ*	Be 18	Mg 48	Zn 130	Cd 224	Hg 400
3	Styptoids,	*Στ*	Bo 22	Al 54	—	—	Tl 408
4	Adamantoids,	*Aδ*	C 24	Si 56	—	Sn 236	Pb 414
3	Phosphoids,	*Φ*	N 28	P 62	As 150	Sb 240	Bi 416
2	Sulphoids,	*θ*	O 32	S 64	Se 158	Te	—
1	Chloroids,	*X*	Fl 38	Cl 71	Br 160	Io 254	—
	Lighter, Earthy—Metals:						
1	Kaloids,	*Kα*		Ka 78		Rb —	Cs 266
2	Calcoids,	*Xα*		Ca 80		Sr —	Ba 274

II. THE IRON-SYSTEM; from Negative to Positive.

		Va 102			
Molybdoids,	*Mλ*	Cr 104		Mo 192	Wo 368
		Mn 110			
Sideroids,	*Σδ*	Fe 112		Ru —	Ir 386
		Ni 116			
Palladoids,	*Πδ*	Co		Pd	Pt 390
Cuproids,	*Kυ*	Cu 127		Ag 216	Au 394

In this work we cannot undertake to enter further upon this subject.

We hope to take up this subject in a separate treatise as soon as time and conditions shall allow.

IV. ATOM-MECHANICS.

But that work thus indicated must be preceded by our *Mechanics of the three States of Matter*, giving the mechanical laws of the fusing and boiling points.

The contents of this work, which has occupied my thoughts for so many years are foreshadowed mainly in the following publications of mine:

Programme der Atomechanik, 1867; 4^0, 44 pp.

Contributions to Molecular Science, 4 Nos., 8^0, 1868, 32 pp; 1869, 24 pp.

The Principles of Pure Crystallography, 1870; 8^0, 48 pp.

Beiträge zur Dynamik des Chemischen Moleküls, 1872, 1873.—Special Edition, Leipzig, 1892; 8^0, pp. 24.

The Principles of Chemistry and Molecular Mechanics, 1874; 8^0, 192 pp.

Sitzungsberichte, K. K. Akademie der Wissenschaften, Vienna, I, vol. 61 and II, vol. 62; 1870.

Comptes Rendus of the Academy of Sciences of Paris, from 1873 to 1900; over thirty notes, 4^0.

Introduction to *General Chemistry*, 1897; 8^0, 400 pp.—Mainly: Lectures, 91 to 100, pp. 350 to 382, and Plates 74–80 and pp. 394–399.

V. EPILOGUE.

In concluding this laborious work, extending over a lifetime, I beg permission to make a personal statement which it will be well for the reader to keep in mind when meeting the Stasian dupes—often in high and influential stations—after the publication of this, our work.

The Work Undertaken.

Ever since I understood the conditions of the chemical elements in reference to a single, primitive substance, (that is, since 1855), I have most faithfully labored in this field, mainly in the following three directions:

I. In the Laboratory by EXPERIMENTATION and in the field and sky by OBSERVATION, the most thorough understanding of the true groundwork of physical and chemical science was sought.

In my Elements of Physics (1870), of Chemistry (1871), and in the method of Quantitative Induction (1872), this groundwork was used in the instruction of very large classes, the largest laboratory classes in America at that time.

II. The great MASTERS OF THE PAST and FOUNDERS OF MODERN SCIENCE were most diligently studied, without regard to difficulties in the way; they became, in fact, my teachers, because I was determined to learn from their works how they solved great problems and how they presented their results.

I trust that I have not been so much with Newton, Kepler, Galileo and Copernicus, without learning something from them by the study of their original publications.

But I have also studied with THE ANCIENT MASTERS, not only with Archimedes and Hipparchus, but also with Plato and Pythagoras.

With admiration and with awe I have learned to read with understanding the most general result of all true modern science in the BOOK OF WISDOM dating back three thousand years:

> "Thou hast disposed all things wisely according to
> "measure, number and weight."

The evolutionist of to-day is welcome to ridicule me for this declaration; he will thereby hit Berzelius just as hard as myself.

Evolution is the "Tischlein decke dich" and the "Eslein strecke dich" of to-day; for its votaries believe that everything just "develops" without an effort or a cause or power; and that this goes on nicely and smoothly, without a jar.

"The survival of the fittest" has become the maxim of personal and national morals. It is very pleasant to the survivor, though he may before God be the greatest brute or criminal.

Such a doctrine is scientifically absurd and morally pernicious.

From day to day the weather gets more and more oppressive—that may be taken as gradual evolution; then comes the conflict in a storm, and purity is restored and vigor returned.

In nations, exactly the same. They gradually progress apparently—but actually get into ruts and become rotten. A war, or a revolution, either awakes their energies and restores them, or subdues and destroys them.

Just so in science. The little new done to-day is magnified a thousand fold by a servile press, and by dependents wishing favors.

Criticism of the living authority is denounced—but the great masters of the past are robbed and insulted with impunity.

Sham-accuracy and show-exhibitions are crowned with honors; while those who, disregarding human authority would seek truth of nature only, are stoned or starved.

We have incidentally been compelled to show such conditions in high circles in some of the capitals of Europe; and we have seen that, even in this direction, America leads the world.

III. Finally, I have for almost half a century diligently taken note of IMPORTANT DETERMINATIONS in the various fields covered by the work imposed upon me.

In this book, I have presented those determinations only which relate to the ATOMIC WEIGHTS OF THE ELEMENTS.

While it was not possible to make the fourth part as complete as intended, I feel confident that no result of value or importance has been omitted.

Truth Denounced and Error Sustained.

With regret, I must admit that I have been positively "*denounced*" for this work* of mathematically demonstrating a great general scientific principle by the facts of experiment and observation.

If our scientific editors and modern authors in science would learn just a little before they teach, write or *denounce*—they would not disgrace themselves before the scientific public of the *Future*.

An Editor of a Weekly Journal of Chemistry, published in London, might be expected to know the name and the character of the work of Newton sufficiently, not to rush a denunciation into his editorial column against a chemist, whose work he does not understand any more than the method and work of Newton, which his denunciation hits fully as much.

The supposition that in these days of vaunted enlightenment and general culture, a new scientific truth, fully demonstrated by established facts, needs only to be published to be accepted, is contrary to experience, which has proved, that scientific authorities of to-day are just as rock-rooted in error and just as prone to denounce and to persecute, as the most notorious bigots and heretic burners of three and four centuries ago.

It is a most deplorable fact that our own once mentally free country has, at public expense of many millions a year, built up the most absolute and most harmful power working for error and enforcing such errors by official National and State authority.

The act of the Secretary of the Smithsonian Institution in officially declaring the false atomic weights of Clarke to

---

* Chemical News, vol. 73, p. 232; 1896.

be true, is infinitely more harmful and condemnable than any action against science ever ascribed to the church or to churchmen.

For the "Smithsonian Institution was founded" for the increase and diffusion of KNOWLEDGE among men per orbem," while the church and its officers have no direct interest in or obligations to scientific knowledge and progress.

But when men who have no understanding of science have the absolute management of great scientific institutions and "bureaus," we cannot expect anything else than what is actually produced, namely, the increase and diffusion of rot and error among men "per orbem."

The Work is Done.

While young and without experience, we suppose that truth only needs to be presented in order to be accepted.

But if this truth be contrary to an error established in the minds of men occupying prominent positions, truth will simply appear as an enemy to these men and will be denounced and fought by them with all the power of their station.

For errors are like thorns and thistles, that grow easily without labor or skill, while truth is a tender plant, requiring careful cultivation by hard labor, to subdue the weeds of error that dispute the ground.

Truly it has been said: "Thorns also, and thistles shall "it bring forth to thee" and "in the sweat of thy face shalt "thou eat bread."

So it has been—so it is—and so it will continue while man remains.

Earnestly have I striven and faithfully have I labored in this vineyard for almost half a century.

May the spirit of truth and wisdom accept the work now done.

THE FIFTEEN CHEMISTS

WHOSE EXPERIMENTAL DETERMINATIONS PERMIT THE ESTABLISHMENT OF THE ABSOLUTE ATOMIC WEIGHTS GIVEN IN PART FOURTH.

Rose, Heinrich. Born August 6, 1795; died January 27, 1864, at Berlin, Germany. Worked in the laboratory of Berzelius. Professor of Chemistry, University of Berlin.

Titanium, 270-271.

Schrötter, Anton Ritter von. Born November 26, 1802; died April 15, 1875, at Vienna, Austria. Professor of Chemistry, University of Vienna.

Phosphorus, 262-263.

Ebelmen, Jacques-Joseph. Born at Beaume-les-Dames, July 10, 1814; died March 31, 1852, at Paris, France. Professor of Chemistry, Ecole des Mines, Director of the Porcelain Works at Sèvres.

Uranium, 271-272.

Erdmann, Axel Joakim. Born August 12, 1814; died December 1, 1869, at Stockholm, Sweden. Worked in the laboratory of Berzelius; later State Geologist of Sweden.

Zinc, 276-278.

Marignac, Jean Charles, Galissard de. Born April 24, 1817; died April 15, 1894, at Geneva, Switzerland. Student of Liebig and Dumas. Professor of Chemistry and Mineralogy at Geneva Academy and University. When, in old age, unable to go to the University, he kept at work in his own laboratory in his own house.

Bromine, 238. Iodine, 258. Also: Pb, 89. Ag, 90. N, 163, 188-189, 192.

Maumené, Edme Jules. Born November 18, 1818. Professor of Chemistry, Institute Catholique, Lyons, France.

Silver, 221-225. Also: Ag, 53. Fe, 93.

Hauer, Karl, Ritter von. Born March 3, 1819; died August 2, 1880, at Vienna, Austria. Chemist of the Reichsanshalt since 1855.

Cadmium, 239-240.

Louyet. Young Chemist at Paris, France; died from the poisonous effect of fluorine, while determining its atomic weight, 1849.

Fluorine, 242-243.

Schneider, Ernst Robert. Born March 20, 1825. Professor extraordinarius of Chemistry, University of Berlin.

The last one, at Berlin, Germany, faithful to Berzelian traditions; hence his great success in atomic weight determination.

Antimony, 265-266. Bismuth, 237. Wolfram, 273-276.

Roscoe, Sir Henry (Enfield). Born January 7, 1833, at London, England. Professor of Chemistry, Owen's College, Manchester (1857-1887); Vice-Chancellor of the University of London (1896).

Vanadium, 273. Also: C, 102, 104. Wo, 274-275.

Winkler, Clemens Alexander. Born December 26, 1838, at Freiburg, Saxony, Germany. Professor of Chemistry at the Bergakademie at Freiburg, since 1873.

Indium, 250.

Nilson, Lars Frederik. Born May 27, 1840, at Skönberga, Söderköping, Sweden. Professor of Chemistry, Upsala University.

Beryllium, 237.

Thorpe, Thomas Edward. Born December 8, 1845, at Manchester, England. Professor of Chemistry, Royal College of Science, London (1885); now Director of Government Laboratories, London.

Silicon, 267. Also: Au, 234. Br, 238. Ti, 270-271. 52.

Pettersson, Sven Otto. Born February 12, 1848, at Göthaborg, Sweden. Professor of Chemistry at the University (Högskola) of Stockholm.

Selenium, 269. Also: Be, 237.

Smith, Edgar F. Born May 23, 1854, at York, in Pennsylvania, United States. Professor of Chemistry, University of Pennsylvania, at Philadelphia (since 1881).

Arsenic, 226-231. Also: Cd, 239. Mo, 260. Pd, 263. Sb, 265-6. Wo, 274-6. 30.

HONOR LIST OF CHEMISTS.

Counting the two German-Austrians to Germany, the one Anglo-American to the English, and the one French-Swiss to France, we find:

The	12 Founders—	2 Swedes,	5 Eng.,	4 Germans,	1 French.
“	15 Part IV. —	3 “	3 “	5 “	4 “
“	27 Chemists—	5 “	8 “	9 “	5 “

This honor list of Chemists having distinguished themselves by reliability and precision in atomic weight work, permitting the establishment of absolute atomic weights by our method, shows some very striking facts greatly at variance with commonly received opinions concerning the standing of the leading Nations in Chemistry.

The Germans are supposed to lead, far ahead in the race. The French admit that (since 1870), and now, even under Moissan, slavishly adopt the atomic weights of the "THREE GERMAN CHEMISTS," and fail to recognize them to be false even after it has been demonstrated by me in their own Comptes Rendus. See pp. 153–157. The English, by the Germans and the French, are supposed to bring up the rear, at a very slow pace; and many of the English, in public print, growlingly admit that position and demand the imitation of German ways. Finally, nobody seems to think that there still are excellent Chemists in Sweden; some leading German Chemists actually publish old Berzelian ideas as "*new discoveries.*"

But the above little table shows that, as a matter of fact, Sweden, in numbers, equals France; in actual work, past and present, it outranks France, as much as Berzelius surpassed Dumas, whose great work of his *early* days is forgotten and even condemned by the present Chemical leaders in France.

And the English growlers at their supposed inferiority, are wonderfully *leading the world* in solid Chemical work. It is true, they have caught the infection from Stas, and their conclusions are sometimes badly in error: *but their actual work, done in their despised little laboratories, is sound and true,* like the old race itself; indeed, I confess that even the aberrations of Crookes (probably due to his early association with Hofmann) are most fascinating, because they are superficial only, not affecting the core, the real determinations, which I have properly recognized as Berzelian (p. 138).

There can be no doubt, to-day THE ENGLISH CHEMISTS LEAD THE WORLD, surely in this, the highest field of Chemistry. The names of Crookes, Ramsay and Rayleigh are stars of first magnitude, and will, we hope, continue to radiate solid and profound truth for many years to come.

The other English Chemists, Roscoe and Thorpe, are fully equal to any worker to-day in Germany. In France, under the scepter of Moissan, we find no serious work done in this field; only false work is awarded academic honors pp. 16, (34, 151-158, 171).

GERMANY, in numbers, barely exceeds England; but its great laborers in this field are all resting from the noble work they have done—and their glory is dragged in the Stasian mire by the living Coal Tar Chemists.

Only Clemens Winkler and Karl Seubert, are working to-day in the way of the old master Berzelius; these two alone, in the magnificently endowed laboratories of the great Universities of Germany. Even the interesting text book of Erdmann does not know the work of Swedish and German Chemists of that great name, but gives the entire modern corruption of Stas, Lothar and Dmitry.

From America, we have been able to admit *only one name* to this honor list, although so many are doing "atomic weight work" as they have learned it in Germany, where already old Chancellor Koch, of Goettingen, declared: Sumimus pecuniam et mittimus asinum in patriam. (Carl Vogt, aus meinem Leben, 1896, p. 138). The influence of this element has been very unfavorable.

This country is too big to remain a dependency of German Chemistry, especially when that Chemistry is such as it is to-day.

The worst permanent damage is done by the encouragement of German ways in University Organization and Scientific Work of the Nation and the States. To copy the German Army System would not prove as bad as what has been and is now being done to the scientific life of America.

What is needed, is a manly independence of thought, not a servile imitation of an Imperial Pattern.

The Imitation of the Royal Institution (only Royal in name, being founded and maintained by sturdy burghers of London) would be more wholesome than the copying of the formalisms of the Universities of the German Empire, with its "theses" and learning in airy heights without visible means of support in nature or in mathematics.

Finally, if we compare the teutonic nations to the romanic, we have twenty-two of the first to five of the latter in our honor list. For the twelve fundamental workers, the proportion is eleven to one.

May this book definitely remove the noxious weeds that have grown around the chemical monument erected by Berzelius, and may the best and truest chemist—also of old Germany—again join in solid work, worthy of the great master: then we shall soon learn the true value of the absolute atomic weight of those elements for which, at present, we lack the necessary experimental data.

INDEX OF NAMES.

lies in the nullovalent elements of the atmosphere (p. 220).

The common gases constitute FOUR DISTINCT STRATA in our atmosphere, of which the estimated thickness is here given in myriameters (about six English miles each). See my papers, Comptes Rendus, August 20, 1900, and National Druggist, Sept. 1900.

I. Lower Atmosphere, with aqueous vapor and carbon dioxide, 2 myr.
II. Oxygen Stratum, 3 myr.; from 16 to 10 % O.
III. Nitrogen Stratum, 5 myr.; from 84 to 4 % N.
IV. Hydrogen Stratum, 7 myr.; from 80 to 100 % H.

The Nullovalent Gases occur according to their densities:

Xenon and Krypton in the lowest stratum, I, exclusively.

Argon terminates in the oxygen stratum, II.

Neon is most abundant in the upper half of the nitrogen stratum, III; while

Helium prevails in the lower part of the hydrogen stratum, IV, reaching about 16 %.

The main physical characters of these gases are: their apparently high conducting power for heat and electricity, together with the brilliant light they emit under high electric tension; this light is marked by characteristic spectra.

The lower gases, Xenon and Krypton, thus must show cloud-formations; the brightest green crypton line of 558 millimicrons is accordingly most characteristic of the lower auroræ.

All these gases will, by their greater electric conductivity, form mobile threads dirigeable by the magnetic force of the earth—as do the iron filings on our glass plate above a common magnet.

From Krypton to Helium, thus beams may form several myriameters in length, constituting great linear conductors from the lower strata to the highest hydrogen stratum.

These beams will show colored light, varying according to the many varied conditions of pressure, per cent amount, intensity of discharge and kind of gas.

This I consider an outline of the mechanism of the Northern Lights.

A paper on this subject was apparently lost in the mails a year ago; hence this short note.